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EDITED BY J. MCKEEN CATTELL

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THE SCIENTIFIC MONTHLY

MAY, 1932

THE UNCERTAINTY PRINCIPLE IN MODERN PHYSICS[1]

By C. G. DARWIN, F.R.S.

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In the course of the last twenty years there has grown up a great body of doctrine, the quantum theory, and its history is in some ways the most curious in the whole of science. The developments of our knowledge have often progressed in an uneven and irregular manner, with important ideas sometimes long overdue before they appear, but at least the progression is usually based on rational assumptions. With the quantum theory this was not so; from its very inception it seemed to run contrary to all logic. Many conservative physicists at first frankly thought that such a self-contradictory theory must be erroneous, but, as more and more phenomena were accommodated, it became impossible to doubt but that there was method in the quantum's madness, and it was possible to anticipate that some time a new synthesis would be made which would remove the contradictions and restore our reason to us. In 1925 the new synthesis was found, and this independently in two quite separate ways. As would be expected, at first the new theory was very abstract; it was, in fact, by denying the validity of the ordinary laws of multiplication that Heisenberg first brought logic into the old confusion, but it was only a couple of years later that Heisenberg himself showed how these rather formal methods would fit into our ordinary processes of thoughts in a natural way. It is with this aspect of the question that I shall be concerned, and in explaining it I shall make more use of the second resolution of the difficulties of the old quantum theory, which was started by de Broglie and worked out by Schrödinger. A great deal of what I shall be saying is to be attributed to Bohr, who more than any other has cleared up the difficulties in which the old theory was involved.

It is evidently out of the question for me to expound all the difficulties of the old quantum theory and then to set them right—to do so would take many weeks. I propose instead to select two experiments, which between them typify the apparent contradictions of the theory, and then to show how the new principle, the Uncertainty Principle, removes the apparent contradictions. My experiments are to be regarded as types, each standing for a great mass of knowledge, and the reconciliation of them carries with it the removal of the main difficulties in the way of our understanding of the quantum theory.

My first experiment is simple, and in one form or another has been done again and again during the last thirty-five years. Take an evacuated tube arranged to carry an electric discharge

[1] Friday evening discourse given at the Royal Institution of Great Britain on February 26.

between a pair of terminals. Cover one part of the glass inside with some powder of a mineral called willemite, and observe what happens in the dark. We should see that the willemite emits faint sparks at intervals all over its surface. A more detailed study shows that each of the sparks is caused by the impact of an electron. An electron is a particle of electricity and is the lightest thing known; it was by experiments of this type that its characteristics, mass and electric charge, were measured. Moreover, the speed of the electrons could be measured in a variety of ways; in some of the experiments it was done by opening and shutting two holes through which the electrons must pass in succession, and timing how long they took between them. To give a general idea of the speed, it is found that when there are 30,000 volts on the tube (not at all a high voltage) the speed is one third of the speed of light. This experiment tells us quite unambiguously that an electric discharge is like a machine-gun firing bullets, the electrons, at a target.

Now turn to the second experiment. I ought, perhaps, to begin by confessing that this experiment has never been done; as I describe it, you will see why it is not practically possible to do it. It is to be regarded as a type-experiment, containing the essential point of other experiments which are practical, such as the great experiment of Davisson, which first established the point I am about to explain. We shall again study an electric discharge of 30,000 volts in a vacuum, but this time with a different outfit inside. We place a shutter in the track of the electrons, containing two small holes in it close together. This is where the practical difficulty comes in, for the two holes are each to be only a thousand millionth of a centimeter in diameter, and they are to be a hundred millionth of a centimeter apart. The tube itself is to be a meter long, and at the end of it, beyond the shutter, we place a photographic plate of extreme sensitivity. After a long exposure (probably centuries would be necessary) we develop the plate. If all this were practically possible there is no doubt what we should observe. The plate would be crossed by alternate bands where it was fogged by the electrons, and bands in between where it was blank, showing that no electrons came there. The bands would be about 7 cms. apart. This is the first half of the experiment. Now repeat it again, but this time with one of the two little holes blocked up. When this plate is developed, we shall find no bands at all, but a uniform fogging all over the plate. Now this is an extremely remarkable fact, when we try to understand how it could come about. Consider one of the blank places on the plate in the first part of the experiment. No electrons came there, and we might try and explain this by saying that in some way the place was protected from the electrons by the shutter. But block up one hole and the place is no longer protected. The addition of what ought obviously to be a further protection has, in fact, had the opposite effect. It is as though the victim of a detective story were invulnerable when sitting in his chair, no matter how many assassins are shooting into the room, as long as he keeps both windows uncurtained, but as soon as the blinds are drawn on one window he falls a victim to the next bullet. This is, of course, fantastic, but it is not more fantastic than my two experiments appear to be at first sight.

As I have stated it, this result seems very remarkable, yet it is a phenomenon quite familiar to any one who has studied physics. It is cognate to one of the great classical experiments of history, the experiment of Thomas Young with light. Young sent light through two

small holes close together (though of course by no means so small or close together as those of our experiment), and observed the actual formation of bands, in much the way I described. He deduced from this that light must be a wave motion, and we must turn aside now to consider what this means.

It will suffice for us to take an example, and the most familiar of all will do, that of waves travelling on the surface of the sea. Imagine that we have a perfectly regular train of waves, crest following crest at regular intervals, advancing across the sea towards a breakwater. First, suppose that there is a small gap in the breakwater, and consider what will happen on the other side. Evidently there will emerge from the gap a train of semicircular waves, with crests all at the same distance apart as before. Next, suppose that we have two gaps in the wall not very far apart. To see what happens I will invoke a very fundamental principle in all wave motions, the principle of superposition, which declares that if there are two motions either of which could happen by itself, then there is a third possible motion of the water, which is simply the sum of the other two; each motion goes on just as though the other were not there. In our case, then, semicircular waves will emerge from each gap, and these are to be superposed. It can be seen that at some places they reinforce one another, but at others they cancel out. This is the phenomenon of interference, and is the explanation of Young's experiment with light.

Our experiment with the electrons has told us that electrons also can show interference, and we conclude that an electron is at any rate sometimes a wave. It is a natural question to ask what sort of wave it may be; for example, the waves we have discussed were waves of the surface of water, sound waves are waves of compression of air, and so on. The answer can best be made by considering the case of light waves. It was natural for Young and the physicists of his time to ask what sort of medium carried the light waves of which he had established the existence, and a special name, the ether, was invented for this medium. But things are not helped on a great deal by the use of this name, since there is one essential difference between the ether and the media carrying sound waves, etc. We can do experiments with water or air which show us that they have properties unconnected with the waves they sometimes carry, but the only property of the ether is that it carries the waves of light. Consequently it is not much use talking about it except in connection with conveyance of waves, and so it becomes really a grammatical construction rather than a physical entity. The same is true for our electron waves, and, in fact, no one has felt the need of a name for the medium carrying them.

We have now got from our two experiments two very different pictures of the electron, in the first as a bullet, and in the second as a wave motion. We cannot see any resemblance between these pictures, and it is our chief aim to-day to see how they can be reconciled. One attempt at reconciliation has been tried several times, but must be condemned altogether. If the electron is sometimes like a piece of grit and sometimes like a stormy sea, the natural compromise is to say that it is really a piece of grit floating in a stormy sea. This is no use at all; like many other compromises it enjoys all the disadvantages of both sides. It is not something in the sea that we need, it is the wave motion of the sea itself.

In order to see how we must proceed, we will first strengthen the contrast by uniting our two experiments into one. In order to do this we replace the photographic plate of the second experiment

by a willemite screen. We should then observe that there were scintillations in the places where the photographic plate was blackened, but none at all in the blank spaces. We can describe what has happened shortly by saying that the electron behaved like a wave in going through the two holes, but changed over into being a bullet when it reached the screen. This is a very unsatisfactory account of the matter, because it attributes foresight to the electron as to what is expected of it, and if we are fanciful we can imagine that we could swindle the electron by painting the two holes so as to look like a willemite screen, and so cause it to be a particle when it ought to be a wave, and absurdities of that kind.

The answer to these contradictions was found by Bohr. It consists in recognizing that experiments that evoke wave characters and those that evoke particle characters are always mutually exclusive, so that the contradictions do not exist in fact; we can not ever deceive the electron by making the two holes look like willemite. For take our experiment with the two holes; this experiment verifies the electron's wave character. We will try to test whether the electrons were particles at the same time, and the best way of doing this would be to decide which of the two holes each single electron went through. To do this we put thin films over the holes, each lightly powdered with willemite; then when an electron goes through a hole there will be a scintillation there, and in this way we can decide through which hole each electron went. We have thus verified the particle character of the electron, and so we now go round behind the screen to verify that the wave character is still all right, but when we do this we are distressed to find that there is no trace of the bands on the plate. A little thought, however, shows that this is natural, for the flashing of the willemite on the hole will certainly cause a reaction on the electron, altering its speed a little (or perhaps only its phase), and this is enough to spoil all chance of interference. Any experiment devised to show the particle character automatically debars us from verifying at the same time the wave character, and *vice versa*. The real point of Bohr's argument is that we now see that this is not a highly abstract doctrine, but almost a matter of plain common sense.

We have got to convince ourselves that the example cited is not exceptional, and that there is no case where we should find a conflict between the demands of the electron that it is to be either a wave or a particle according to the experiment to which it is submitted. But it will be best for the moment to assume this and to construct a view of nature which will enable us to think about the way things behave. The best view, I think one might say the only view, that one can adopt is that there is in nature a fundamental duality, typified by the characters of wave and particle. An electron, and, in fact, any other piece of matter too, is a wave and a particle, and both characters must be kept in mind, but they must not be mixed. Some experiments evoke the particle characteristics, such as striking the willemite, and others the wave character; and whichever type of experiment is carried out it is this experiment that brings into prominence the corresponding character. We shall have to see later how it is possible that there is no contradiction in such ideas, but first we had better examine the nature of the duality itself, for it is something quite new in scientific thought. I think it should be admitted frankly that we were all quite happy before the duality was discovered, and that it would have been a simpler world if it had turned out that we did not need it, but there it is as the only way in which one can understand the

fundamental difficulties of atomic theory. There is nothing like this duality anywhere else in science, but it may perhaps make it more acceptable if I recall that there is a duality in metaphysics which we all accept cheerfully and without complaint. I refer to the duality between the objective and the subjective, between, for example, the rather difficult abstractions of the scientific conception of heat and the much more poignant ideas we have when we burn a finger. Or, again, there is a very complete separation between the idea of light of wave-length, one-twenty-thousandth of a centimeter, and the perception of green light, and yet the one always causes the other. There is the same sort of interdependence, associated with the use of two entirely different languages, in the particle and wave aspects of the electron. It should, perhaps, be confessed that the analogy which I have drawn is not a close one, for the conceptions of metaphysics are not comparable to those of science, but nevertheless it is perhaps helpful as showing how completely and spontaneously we accept, and can not help accepting, one duality, and so perhaps it may mitigate our difficulty in accepting another.

We must now see how the characters of wave and particle are to be associated with one another. The main character of a wave is the wave-length, the distance between successive crests. On the other hand, the main characters of a particle are its speed and position. There is a very simple relation between the speed of the particle aspect and the wave-length of the wave aspect; they are inversely proportional, so that the slower the electron the longer the wave-length. For our 30,000 volt electrons the wave-length, as calculated from the breadth of the fringes, will be very small, less than a thousand-millionth of a centimeter. This is for electrons with speed a third that of light, and it is mostly with these high speeds that the theory has been tested, since only so have the electrons enough energy to fog the plate or produce scintillations. But if we have much slower electrons we get longer wave-lengths; for example, an electron going at the rather slow rate of 1 cm. a second has wave-length about 7 cms.

This is the formal relationship, and I am not, of course, asserting that one could see a wave with crests 7 cms. apart under any conditions. What is asserted is that one would find an exact geometrical similarity between our experiment of the two holes and an experiment with ripples 7 cms. long on the surface of water; if these went through two gaps at the same distance apart the disturbed regions would be found at the same positions as those where the electrons appear. But now let us take our connections of the two aspects more literally. A wave of 7 cms. wave-length does not by any means imply two crests 7 cms. apart; it means a regular train of waves with crests spaced at 7 cms. intervals stretching to infinity in both directions. We have seen that such a wave system corresponds to a particle moving at a rate of 1 cm. a second, but that gives no information at all about what is the most natural and reasonable question that we may ask—where in all this wide sea is the particle? It was the finding of the answer to this question that cleared the whole business up. The answer is that it may be absolutely anywhere. This was quite unforeseen, and seems at first sight contrary to common sense—for, after all, the electron must be somewhere, and there ought to be something corresponding to this fact in the wave aspect. It is the fact that it does make sense that is the point of the uncertainty principle.

To understand it, let us take a slightly different case, since a wave motion stretching to infinity is rather overwhelming.

Take the case of a "wave packet" composed of a number of crests at equal intervals, surrounded by quiet water. If this represents an electron wave, where is the electron-particle? The answer is that though we can not say where it is, we can say where it is not, for it can not be in the undisturbed region outside the packet; if the idea of waves is to mean anything, we must at least suppose that where there is no wave disturbance there can be nothing happening, and so no particle. We must next make a digression to consider how such a wave packet will behave with the lapse of time; this is a well-known and much-studied question of wave motions in general. It is found that a patch of waves travels along in a straight line at a definite rate depending on the wave-length, but that as it goes it spreads so as to increase slowly in size. The rate at which it spreads depends on the length of the packet; a long packet, containing many crests, will only spread slowly, while if it only has a few its spread is very rapid. For our electron we are supposing that the particle is certainly at first somewhere in the packet; where is it later? Evidently still in the packet, so it must have travelled with the speed of the packet. But here comes an important point; the particle's speed can not be fixed accurately in this way, because the packet is spreading. Thus if we imagine that the particle was moving at the head of the packet it would be going faster than if it were at the tail; and it would be going still faster if it started at the tail and ended at the head. So our idea that the particle may be anywhere in the packet carries with it the implication that the speed is to some extent indefinite. In this way we see that our idea of duality will only work on the condition that there is some indefiniteness in the position and speed of the particle.

The indeterminacy can be expressed in a more definite form. Thus in a very long wave packet the position of the electron is very uncertain, but as such a packet does not spread much the speed is rather precise; on the other hand, a short wave packet will clearly define a rather accurate position for the electron, but such a packet spreads very quickly, and this implies great uncertainty in the speed. It is possible to give rough numerical limits to the two uncertainties; suppose that we measure position in centimeters and speed in centimeters per second, then it happens by a sort of numerical accident that for electrons the product of the two uncertainties is about 1. For example, if I know for sure that an electron is somewhere within a distance of a hundredth of a centimeter from some place, then the uncertainty principle asserts that its speed must be uncertain to about a hundred centimeters a second, so that it would only stay for sure in the region where I had located it for a ten-thousandth of a second. At first sight this seemed a fantastic result, and it is little wonder that it was only recently discovered that it is entirely sensible and natural.

In order to justify our dualistic hypothesis then we have got to show that there is something wrong with the simple idea that we could measure anything we please as accurately as we please. We shall discuss the case of the electron because on account of its being the lightest thing in the world, it provides the most crucial case; the uncertainties for other bodies are always less, and are small beyond conceivable observation for bodies of ordinary size. We have got to show that it is not possible to measure simultaneously the position and speed of an electron with precisions higher than those allowed by the uncertainty principle; and we must recognize that if any one can contrive a method by which he can better these precisions, then the whole structure of modern physical

theory collapses, and we know nothing about the foundations of nature. It was the great achievement of Heisenberg to show that all is well with our theories.

We have to devise an experiment which is simultaneously to fix the position and velocity of an electron with the highest possible precision. The easiest way to fix a position with accuracy is to take a microscope, and we can simplify the question of speed by supposing the electron motionless. Our experiment then consists in looking through a microscope at a stationary electron; it is not actually a practical experiment, though very nearly so, but that does not matter because our object is to prove that it would not work with unlimited accuracy even if it were practical. We now have to consider the question of the accuracy of our instrument. The question of speed is easy, because, if the electron stays near the same place for a considerable time, it obviously has very small speed, and so we can check the speed to be zero as closely as we like by taking a long time over the observations. As to the accuracy with which we can observe the position, I must cite certain well-known characteristics of the microscope. If we take the highest possible power of a microscope, and look at a series of objects each smaller than the last, we find that though the larger ones can be seen in sharp detail, when we come to the smaller ones that is not so—their sharper corners seem to be rounded and hazy; and finally we get down to objects which can only be seen as round discs, though they may really be as angular as the larger ones. This failure is easily explained from the wave theory of light; it is never possible to see detail of a scale smaller than the wave-length of the light illuminating the object. The wave-length of visible light is about one-twenty-thousandth of a centimeter, and so if we are to use ordinary light we certainly can not decide the position of our electron more precisely than this. We can do better, however, by imitating the process used in microphotography, which considerably extends the range of visibility by the use of ultra-violet light. But whereas in the practical technique of the microscope it is hardly possible to double the resolution by these means, we are not so limited in our idealized experiment; we may take a wave-length many thousand times smaller. For our purpose the x-rays would be good, but still better are the γ-rays of radium, which have a wave-length about a hundred-thousandth of that of visible light say, roughly, a ten-thousand millionth of a centimeter. With a microscope using this illumination we really can do something. But when we set the apparatus up we find a curious thing; the electron may be there at the beginning of the experiment, and may be at rest, but something always happens so that the electron gets knocked away, and our experiment is ruined every time. We must see how this comes about.

About nine years ago, A. H. Compton was led to consider the question of the way in which an electron would react to light. This is, of course, an old problem, but the virtue of Compton's method was that he regarded it is a collision of two bodies, a light particle and an electron-particle. This was long before the wave aspect of the electron was dreamt of, and though the particle aspect of light was known, no one had as yet taken it so seriously. He found that an electron can only scatter light by itself being set in motion, and worked out the various relations for the scattering of the light and the recoil of the electron. He then verified his predictions very fully, so that in speaking of the Compton effect we are not referring to an abstract doctrine but to hard fact. Compton showed that the recoil of the electron is insig-

nificant for visible light, is easily perceptible for the shorter wave-length of x-rays, and attains very large values if the light has the extremely short wave-length of the γ-rays.

We can now see where our experiment is going to go wrong. We may compare our problem to that of the biologist who wants to study the life-history of some cave-dwelling animal which dies if the faintest light falls on it; the animal is killed by the mere attempt to see how it lives. In our case there is no difficulty about imagining that an electron-particle is absolutely at rest in some absolutely exact place, but it is useless to imagine it because it is unverifiable. We can not verify that it is there without light to see it by, and if we want to know the position with high accuracy we must use γ-ray light. But this light can only reveal the electron provided it is scattered by the electron; and when it is so scattered there is bound to be a recoil, so that even if the electron was at rest before, it will not be at rest after the scattering. So we can not fix the position exactly without thereby spoiling the velocity. If, on the other hand, we are content with a rather rough knowledge of the position we might use ordinary light in our microscope, and thus only get a very small recoil; we have sacrificed knowledge of position and have gained a greater precision in our knowledge of the speed. When it is worked out in detail, the result of our experiment is in exact conformity with the uncertainty principle; that is to say, we can fix either position or speed as accurately as we please, but either only at the expense of the other.

The microscope is evidently not going to help us in breaking the uncertainty principle, because of the tiresome way in which the light makes the electron recoil, and we therefore try again, this time without the use of light. Suppose, for example, that we have a source of electrons to the left of two screens. The screens have very fine holes cut in them, which are closed with shutters most of the time. We first open the left-hand shutter for an instant, and then the right-hand one for an instant. Any electron which is found to the right of both screens must have come along the line of the holes, and must have taken a definite time in doing so. Surely we now know position and velocity as accurately as we like, by making the holes very small and only opening them for very short times. Quite true; we can say where the electron was, but not where it is, since on its emergence from the second hole the electron will be scattered into some arbitrary direction, like the wave going through the gap in the sea-wall. We have, in fact, only succeeded in practicing the noble art of foretelling the past. Once again we see that our idea of the particle as occupying a sequence of exact positions is a useless one, because the experiment devised to tell us what the position is will always disturb the particle giving it a velocity that it would not have had if we had not made the experiment, and so making the determination useless.

The general outcome of this and other experiments is always the same, and may be summarized thus. If we were conservative and did not believe in all the new ideas, we should regard the electron as a little piece of grit, and therefore located at some precise place and possessing some definite speed of motion all the time. But in adopting this belief we lay ourselves open to the challenge of being asked to prove what the position and velocity are; and when we come to examine the details of the appropriate experiments, we find that the values will always elude us, and remain undecided to just the extent suggested by the uncertainty principle. There is, therefore, no warrant for our conservative assumption, and, on the other hand, there is no

contradiction in the dualistic ideas of wave and particle. Before going on I ought to say that I have simplified the question by only discussing the case of the electron. The uncertainty principle applies to everything else as well, and the only reason why I have not discussed the other cases is that the electron provides the extreme case, so that if it can be met there, all the others are easy.

The uncertainty principle has very radically altered our views about one of the fundamental principles of knowledge, the principle of causality. We used to think that there was an absolute causality in the world; that a full knowledge of the present would inevitably lead to certainty about the future. It is true that the force of the principle was much reduced by the fact that we usually had to confess to so much ignorance about the present that we could excuse ourselves from any great self-confidence in prophecy; but we used to feel it as a slight reproach that we suffered from this ignorance, and to imagine that our descendants would become perfect prophets because they would be omniscient. But now we see that the present is definitely unknowable, so that there is no possibility of foretelling the future with certainty. Every time we determine something in the present we spoil something else; and we are always at least one step behind what we need for the purpose of confident prophecy. Instead of certainties we have to deal with probabilities, and the theory of chance is raised to a more dignified position than it used to occupy; it is no longer a confession of ignorance of things we feel we ought to know, but of things essentially unknowable.

Closely allied with the question of causality is the question of free will and determinism, which has puzzled the philosophers of all ages. It is really not a matter for the scientist, but it is impossible to conclude without a reference to it. What I am now saying has none of the authority of scientific knowledge behind it, and it is improbable that any one will agree with it. I can lay no claim to philosophic knowledge, but my views certainly resemble those held by some philosophers in this, that they change every few months. In spite of the great effect that the new theory has on the idea of causality I believe it leaves the question of free will where it was.

There appear to me to be two radically different kinds of free will, which may be called *my* free will and *your* free will. When I think about my free will, I conceive myself standing right outside the operations of nature, making arbitrary decisions without reference to causes of any kind, such as the decision whether I shall raise or lower my hand. When I observe your free will, it seems to me to be a very inferior thing, a certain capriciousness in your actions, for example a perfect uncertainty as to whether you will raise or lower your hand; in fact, though you claim that you are really displaying my kind of free will, yet as far as my observation goes your actions have the same sort of uncertainty as that displayed by an electron. It used to be felt by some that the old inexorable laws of nature allowed no latitude for this capriciousness, and that now that we have found that those laws were wrong, we have escaped from the rigid determinism that they implied. I do not think this is the real point. Though the behavior of a single electron is very uncertain, that of a thousand will be fairly regular, and when we come to the large number of electrons in the human body we expect a very complete regularity. The human body will be governed by the new laws of nature almost as rigorously as by the old, but when we consider the enormous complication of a human being, and the complete impos-

sibility of knowing even roughly what all the parts of the human machine are doing, this rigor need not trouble us, for even before the advent of the uncertainty principle our ignorance was surely ample enough, and was evidently always going to be ample enough, to prevent any confident forecast of our actions. So I conclude that there was no great difficulty about *your* free will even before the discovery of the new principles, and that the philosophic difficulty is just what it was before, the intolerable contradiction between *my* free will and *yours*.

It is possible that these difficulties will always remain to puzzle us, but it really does not seem to matter very much whether they do, whatever their appeal to the introspectionist may be. It is the scientific meaning of the new theory that is of permanent value. The earlier direct experiments with atoms revealed certain contradictions. The new quantum theory removed these contradictions, but seemed at first to be composed of the most fantastic abstractions. It was the uncertainty principle that revealed the common sense of these abstractions.

SOME ATTRACTIONS OF THE FIELD STUDY OF ANTS

By Dr. WILLIAM MORTON WHEELER
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After more than thirty years devoted to the study of ants I find myself wondering why so very few of our nature lovers have engaged in this pursuit. I find that observing and collecting ants in many lands, quite apart from the benefit to my health, have yielded a keen delight which has remained with me and seems to have colored my recollections, so that I have acquired the habit, when regrets and unpleasant memories assail me, of effacing them with the memories of excursions in mountains, forests and deserts peopled by colonies of thrillingly interesting ants. Certainly the pursuit of any branch of natural history may be recommended as an avocation to our youth, to convalescents, to our tired business men or in fact to any one who craves a hobby, a surcease from the nerve-racking routine of our city life or a valid excuse for remaining as many hours as possible in the open air of the woods and fields.* But no branch of natural history, in my opinion, is so well adapted to furthering these ends as myrmecology. I shall therefore discuss some of the advantages of ants as material for observation and study, and add a brief account of the simple equipment needed by the field and laboratory observer in the hope that some of my readers may be persuaded to join the ranks of the myrmecologists. In conclusion I shall call attention to a few of the many problems to the solution of which the patient and enthusiastic student of these insects may hope to contribute.

The ants, unlike many other insect families, are represented by comparatively few species in any given locality, but, being social organisms, this deficiency is compensated by the great numbers of individuals even in very circumscribed areas. The observer, therefore, always has an abundance of material at his disposal without being confused by such a maze of species, represented by comparatively few individuals, as he would encounter in many families of flies, beetles, moths, etc. The number of species, subspecies and varieties of ants scarcely exceeds 60 in any of our New England states, and the entire ant fauna even in any one of our Southern or Southwestern states does not exceed 100 to 150. The fact that ants can be so easily collected, since their most abundant caste, the workers, are wingless and the males and females take to their wings only during the marriage flight, may also be cited as greatly facilitating their observation and study. Nevertheless, fear seems to have prevented many people from making an intimate acquaintance with these insects, though few of our North American forms can sting or bite painfully and the great majority of them are only feebly aggressive or even decidedly timid. Probably most people are so annoyed by the sensation of ants

*As the Australian myrmecologist, Mr. John Clark, remarks: "The study of ants is most interesting, and entails very little exertion. It should appeal to those whose health does not allow of vigorous work in the bush. It keeps the observer in the open, with his mind fully occupied, so that life's worries are soon forgotten, while a store of valuable information is gained. Ants are numerous everywhere. They are easily kept in artificial nests, and make interesting pets. The food required by them is always at hand, and the nests are readily made; so that no one should experience much difficulty in keeping ants for observation at home."

crawling over the skin that they fear to disturb colonies. This aversion, however, is soon overcome. There are, of course, certain stinging species like the agricultural ants (Pogonomyrmex) of our Western states, the driver ants (Dorylus) of Africa, the legionary ants (Eciton) of Tropical America, and the bull-dog ants (Myrmecia) of Australia, which make much greater demands on the fortitude of the observer. Excepting in such species, however, which equal or even surpass the honey-bee, bumblebees and wasps in aggressive and stinging behavior, the economy of ants, as compared with that of very many insects, is singularly open to observation. The structure of their nests, the whole personnel of their colonies, the peculiarities of the various castes of adult ants, all the different stages of the brood from egg to pupa, can be examined without any difficulty. The marriage flights of the males and females, the expeditions and wars and the founding and development of the nests and colonies also furnish inexhaustible material for study in the field.

Ants are so extremely sensitive to the degrees of temperature and humidity of their environment and to the character of its vegetation that many species or subspecies are confined to very narrow ecological habitats. This specificity of adaptation also furnishes interesting matter for study, especially in connection with the fact that nearly all species of ants are highly variable and exhibit many subspecies or geographical races and even more numerous varieties characterized by subtle peculiarities of color, hairiness and sculpture. Hence the great importance of the ants as material for the investigation of geographical distribution, variation and polymorphism. An even more fascinating field for observation is afforded by the intricate relations between ants and other ants and between ants and other organisms, both animal and plant. To the former relations belong the phenomena of inquilinism, social parasitism and slavery, to the latter the relations of ants to certain peculiar plants (myrmecophytes) and myrmecophily, or the extraordinary relations of the ants to their insect guests, parasites and "cattle" (plant-lice, scale-insects, certain butterfly caterpillars, etc.). Field observation thus broadens out into a study of great scope and complexity and comprises many cases of the most marvelous behavior, both on the part of the ants and of their charges, known to occur among insects.

Finally a word may be added concerning the esthetic aspect of myrmecology. Ants are not usually regarded as beautiful insects, and this is probably responsible for their neglect by many entomologists. There are, however, in the tropics some very beautifully colored forms, notably in Cuba and Australia, which possess a number of gorgeously metallic green or purple species, but those of temperate regions are apt to be merely dull yellow, reddish, brown or black. Yet even these species when viewed under a low magnification are by no means unattractive. Indeed, the form of the body, especially of the workers and males, is decidedly graceful, and the texture of the integument, with its endless variety of fine sculpturing, polish and luster and its delicate pilosity is in many species exquisitely beautiful. Unfortunately, we possess no really artistic illustrations of ants, like the famous pictures of birds, beetles, butterflies, etc., because as yet no gifted artist has interested himself in these insects.

Since field observations on the behavior of ants can be of little value without a recognition of their various species, subspecies and varieties, it is necessary to collect and preserve specimens for comparative study and as essential records of observation. This taxonomic or classificatory aspect is less irksome in

myrmecology than in the study of many other groups of insects for the reason, already mentioned, that the various forms occurring over a considerable area are not very numerous. The field worker will readily become acquainted with the majority of the forms in his state during a single season, but it may require several years for him to find all the rarer forms that nest in concealed situations. Often their colonies can be located only by carefully following accidentally encountered worker individuals till they return to the nest and thus betray its site. Even this method is impossible, however, in the case of certain minute subterranean forms, which rarely or never appear on the surface of the soil. These can be secured, as a rule only by using the Silvestri funnel described below.

The apparatus for field work in myrmecology is very simple and easily obtained. It comprises the following articles, all of which can be carried in the pockets of an outing jacket.

(1) Some cloth bags, each capable of containing about two or three quarts of earth.

(2) A large white handkerchief or a piece of white oil-cloth about a square yard in area.

(3) A number of vials of ethyl alcohol, or if this can not be readily obtained, of methylated or denatured alcohol.

(4) A pair of tweezers with fine, or preferably, with smooth, narrow, flattened points.

(5) A short, strong chisel, with a blade about an inch in diameter, a small trowel or a strong-bladed plasterer's knife.

(6) A good pocket-lens. An excellent one, with magnifications of 10 and 20 diameters is supplied by the Zeiss or Leitz optical companies.

(7) A note-book.

The uses of these articles, with the exception of (1) and (2), will be obvious. An ant nest is dug up with the chisel or trowel and the earth, detritus or dead wood quickly thrown on the piece of cloth which has been spread on the ground. The material of the nest can then be carefully broken up and the ants or any other insects associated with them collected with the tweezers before they can escape. The bags are used for holding entire small or selected portions of large nests which are to be carried home or to the laboratory for more careful inspection and collection than is usually possible in the field or for instalment in artificial nests.

To the articles listed above two pieces of apparatus, the "exhaustor" and Silvestri's modification of "Berlese's funnel," have been added very recently. They are so useful to the field myrmecologist that I here figure and describe them in greater detail. The exhaustor (Fig. 1) consists of a strong, wide-mouthed vial (A) furnished with a cork or rubber stopper (B) perforated for the accommodation of two tightly-fitting

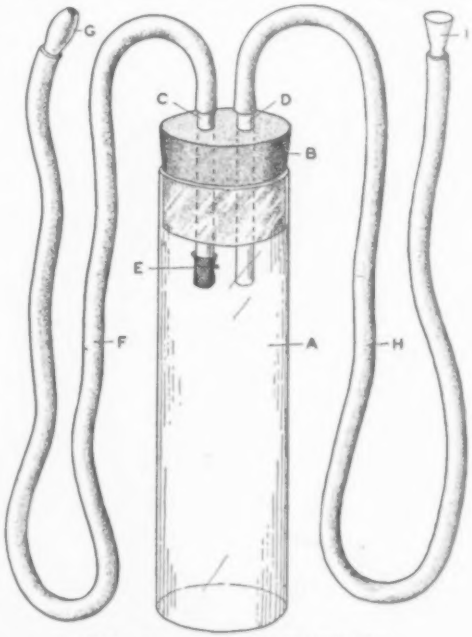

FIG. 1. THE EXHAUSTOR

glass or metal tubes (C, D). The upper end of each of these tubes is fitted into a flexible rubber tube 12 to 18 inches long, one of which (F) ends in a perforated mouthpiece (G), the other (H) in a small glass funnel (I). Neither of these attachments, however, is really necessary. The lower end of the tube C is covered with a cap of gauze (E). If this tube is made of metal instead of glass a small piece of fine wire netting soldered to the walls of its opening may be substituted for the gauze. In using the exhaustor for collecting ants and other small insects running over the ground, tree-trunks or foliage the vial is held in the left hand, the mouthpiece is placed between the lips and the opening of the funnel or the end of tube H is placed over the insect, which can then be sucked into the vial. Large numbers of extremely delicate Arthropods, such as small beetles, Pauropods, Thysanurans, springtails, mites, gnats, ants, etc., which are apt to be more or less injured if picked up with the tweezers or even with a brush dipped in alcohol, may thus be captured in perfect condition. If the collector carries a supply of vials of the same size as that of the exhaustor he can, after making a collection, remove it from the cork and substitute a fresh vial as often as desired. The captured insects can be either kept alive or killed and preserved by pouring a small amount of alcohol into the vial. The exhaustor seems to have been invented by the economic entomologists to avoid handling delicate insects with the tweezers or fingers. It should be of considerable use in physiological experimentation whenever it is necessary to move insects about without injuring their delicate wings or articulations or infecting them with foreign odors. The exhaustor is also very useful in transferring living ants from one artificial nest to another. A little practice with the instrument in the field will enable the collector to capture a great number of uninjured specimens in a very short time compared with the old and laborious method of picking them up one by one with the tweezers.

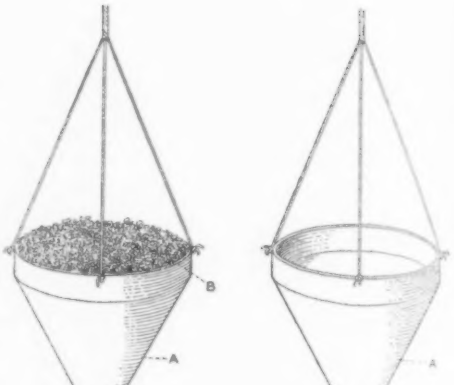

FIG. 2. SILVESTRI'S FUNNEL

The Berlese funnel is an apparatus which collects minute Arthropods automatically from samples of humus or soil. In its original form it was rather cumbersome and not easily manufactured, but Silvestri has simplified its construction without impairing its efficiency. I translate his account and reproduce his figure (Fig. 2).[1] "The apparatus suggested by me was described by Mr. E. Jacobson[2] after he had used it for several years according to my directions. It is

[1] F. Silvestri. Aparato para recolección de pequeños Artropodos. Conf. y Res. Real Soc. Española de Hist. Nat. Madrid, p. 10–13, 1 fig., 1930.

[2] E. Jacobson. Hilfsmittel beim Fang und Präparieren von Insekten, besonders in den Tropen. Deutsch. Ent. Nation. Bibliothek 1, pp. 94–95, fig. 8, 1910.

very simple, consisting of a funnel (A), which can be suspended by three cords wherever desired; of a sieve (B), with meshes one to two millimeters in diameter, and a glass vial (D) containing alcohol and attachable to the end of the funnel by means of a piece of rubber tubing (C). This apparatus, thus simplified, is very convenient, especially for traveling naturalists, because they can have two or more funnels made, of slightly different diameters, so that they can be fitted one inside the other and thus occupy but little space. In my travels I have always had with me since 1908 three of these funnels, which have never caused me any inconvenience. . . . This apparatus may be made of sheet zinc or, more economically though less durably, of tin. It requires no attention after the selected material [humus, detritus, soil, etc.] has been placed in the sieve, [this set in the top of the funnel], and the vial of alcohol attached to the end of the funnel. The length of time the material should be left in the sieve is very variable, depending on the quantity and humidity of the material and the temperature and humidity of the surrounding air." With the gradual drying out of the contents of the sieve, the minute insects fall one by one into the funnel and thence into the vial of alcohol at its lower end. "This simple apparatus," as Silvestri states, "should be used by all entomologists interested in the microgenton and especially by those who have an opportunity to live for some time in tropical regions where it is still possible to discover many interesting Arthropods." He coined the term "microgenton" to embrace all the minute, poorly pigmented and usually blind Arthropods that inhabit vegetable mold, soil, dead leaves, decayed wood, etc. Quite a number of our smallest ants belong to this remarkable ecological association and are very difficult to collect without the aid of the funnel above described.

Perhaps in this connection another and very different piece of labor-saving collecting apparatus should be mentioned, namely, the automobile. Its value to the field myrmecologist especially, in enabling him to visit remote and unfrequented territory without fatigue and loss of time, can not be exaggerated. It will probably be found that both private and museum collections have greatly increased the number and variety of their accessions, especially of insects and other invertebrates, since the automobile came into general use.

The myrmecologist will be led naturally to supplement his field observations with observations in the laboratory. Though this may be as unpretentious as any small well-lighted room in a house, he will wish to equip it with the apparatus both of the entomologist—insect boxes, insect-pins, labels, etc., and those of the histologist—compound microscope, binocular dissecting microscope and the usual glassware and reagents. He will also need a number of artificial nests for living colonies of ants. I have described some of the various patterns of these in an appendix to my ant book,[3] and a few others in a note published in 1910.[4] During recent years I have found that colonies of small ants can be kept for some time in ordinary Petri dishes, pro-

[3] W. M. Wheeler. "Ants, Their Structure, Development and Behavior," Columbia Univ. Press, N. Y. 1910; reimpr., 1926.

[4] W. M. Wheeler. "Small Artificial Ant-nests of Novel Patterns," *Psyche*, April, 1910, pp. 73–75, 1 fig. There is an excellent chapter on artificial nests and on collecting and preserving ants in H. Donisthorpe's "British Ants," 2nd ed., 1917. The following recent literature on ant-study and artificial nests may also be consulted with profit: H. Kutter, "Züchtung von Ameisen," in E. Abderhalden's "Handbuch der biologischen Arbeitsmethoden," 1926; A. Stärcke, "Ein neues Formicarium," *Zool. Anzeig.*, 92, 1930, pp. 152–155; H. Eidmann, *Zeitschr. f. vergleich. Physiol.*, 3, 1926, pp. 776–821, and 7, 1928, pp. 39–55; E. Meyer, *Biol. Zentralbl.*, 43, 1923, pp. 353–404 and 47, 1927, pp. 264–307.

vided the optimum supply of humidity and food is maintained. The colonies can be readily transferred when required from fouled to clean dishes, by means of the exhaustor.

The beginner in myrmecology may become disconcerted or even depressed when he discovers what an enormous mass of literature on the ants has been produced during the past two centuries. The veteran investigator may, perhaps, encourage him with the statement that we are only just entering on the more experimental and statistical and therefore more serious, interesting and enlightening study of these extraordinary insects. It is generally agreed that ants communicate with one another, but the precise character and scope of this behavior in the various species have never been satisfactorily ascertained. The same is true of the homing behavior, though it has been the subject of much valuable experimental investigation. We are still very much in the dark in regard to the feeding of the larvae by the workers and therefore of the precise method of determining the development of the various castes (polymorphism), which are such a striking feature of the ant community. Our knowledge of the parasitic and slave-making ants and of the interrelations of ants and myrmecophiles remains very sketchy. We possess no satisfactory information concerning the factors that initiate and control the movement of ant colonies to new nesting sites. The population of an adult ant-colony seems to be a true specific character, but we have almost no statistical data on the number of ants in fully developed colonies in various species, on the rate of the development of colonies or on their life-span. Moreover, certain investigations which have been carefully pursued in Europe have never been extended to our American ant fauna, such as those relating to the interesting transportation and distribution of plant seeds (myrmecochory). This list of matters awaiting further elucidation might be indefinitely increased. For the solution of those mentioned the ant-fauna of the United States, which is so much richer than that of Europe, should afford adequate material, without resort to the splendid but less easily accessible neotropical fauna of the West Indies, Mexico and Central America. We only need more myrmecologists.

CANNIBALISM AMONG THE SHARKS AND RAYS

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THE elasmobranchs are the lowest of the great group of backboned animals. They have no ribs and their skeletons, instead of being bony, are made up of cartilage. The higher fishes have their gills in one gill-chamber covered by a bony operculum or cover with one opening for the discharge of water. The elasmobranchs have no gill cover, but each gill has a separate opening. These openings are separated by straps of skin and flesh, hence the name elasmobranch (strap-gilled).

It has long been known in a general way that these animals feed on each other, but no attempt has ever been made to set the facts out in order. How I have been led to take up this (to me) interesting task will be indicated below.

SHARKS FEEDING ON STING-RAYS

Everywhere that I have fished for sharks, my fishermen helpers have advised the use of the flesh of sting-rays as the best available bait. Indeed at Key West and at the Marine Biological Laboratory of the Carnegie Institution of Washington, at the Dry Tortugas, Florida, where I fished rather extensively for sharks for four seasons (1912 to 1915) we used to hunt for sting-rays to use their fins as bait.

With this sting-ray bait we always had better success than with anything else. A professional shark fisherman, Captain W. E. Young, of Brooklyn, N. Y., tells me that he always uses ray bait when possible. He has for many years fished for sharks all over the world and has more first-hand knowledge of methods of catching these fish than any man known to me. However, I had long known that at least one kind of shark was very partial to sting-ray flesh. This I reported in *Science* (Vol. 25, pp. 1005–1006, 1907).

THE HAMMER-HEAD SHARK A STING-RAY EATER

Late in July, 1906, numbers of large sharks were seen in the inner harbor of Beaufort, N. C. On July 20, a big hammer-head (*Sphyrna zygaena*) was seen chasing sting-rays over the sand flats. Presently it came near an anchored sharpie and was harpooned, but the "iron" pulled out. However, when it chased the sting-ray across the bow of the boat again, it was again harpooned, and this time the harpoon held and the shark was secured. Eighteen hours later I purchased the fish, towed it to the laboratory of the U. S. Bureau of Fisheries, hoisted it up on a davit on the wharf, and there skinned and dissected it. This huge specimen was 12 feet, 6 inches long; 4 feet, 2 inches in girth; 3 feet wide between the eyes; and had the dorsal lobe of its tail measuring 3 feet, 6 inches in length. Fig. 1 shows it suspended from the davit ready for skinning and dissection.

This specimen was a female and was dissected in the hope that embryos might be found in the oviducts. In this matter I suffered a keen disappointment, but on opening the stomach I was surprised to find the almost perfect skeleton of a sting-ray, together with cartilaginous fragments of other such skeletons. Furthermore, when I began to work the skin away from the lower jaw and throat region of the shark the most surprising thing of all came to light—these parts

FIG. 1. A LARGE HAMMER-HEADED SHARK

THIS 12-FOOT, 6-INCH SPECIMEN OF *Sphyrna zygaena* IS HERE SHOWN SWUNG UP BY A DAVIT AT THE WHARF OF THE LABORATORY OF THE U. S. BUREAU OF FISHERIES AT BEAUFORT, N. C. NOTE THE MALLET-LIKE PROLONGATIONS THAT GIVE THIS SHARK ITS NAME. THE EYES ARE AT THE OUTER EXTREMITY OF THESE.

were found to be full of sting-ray stings, one even being found in the back. Later, when the jaws were being cut out and cleaned up, they were found to be a perfect mine of stings.

On the day following the removal of the skin, about forty hours after the capture of the shark, I began the work of cutting out and cleaning the jaws. With the July temperature at Beaufort well up in the eighties, the shark was in a pretty "high" condition, so the jaws were cut out and the shark thrown overboard. The jaws were then carefully dissected. In cleaning them more than 50 stings were found embedded in the muscle masses adherent to the jaws, and stuck in the membrane covering over the cartilages of the jaws. Sometimes as many as three or four tips of stings were found in a cube of flesh an inch square and two inches long. Furthermore, the lower jaw cartilages from one corner of the mouth to the other were scarred and ridged—evidently the results of former combats in which the rays had used their spines effectively.

A total of 54 fragments of sting-ray stings were found. These vary from large pieces three and four inches long (at least half the spine) to small points only one half inch in length. In addition, four pectoral spines of the gaff-topsail catfish were found. These 58 spines were taken mainly from the jaws and the adherent flesh. Some, however, came from the neck region. The flesh around some of these stings was still full of congested blood, showing that the stings were but newly implanted. Others (especially those piercing the membranes covering the jaw cartilages) were embedded in cysts, showing that they had been carried for a long time. When this very extensive "mine" had been "worked," I greatly regretted that, despite its offensive condition, the body had not been kept and thoroughly dissected for spines. Had this been done undoubtedly many more stings would have been found in the neck and chest region.

Fig. 2 shows the 54 fragments of sting-ray stings and at the top the four pectoral spines of the ocean catfish (*Felichthys felis*) which were collected. All the stings were broken, save the one with the forked base, which was torn out intact. It is from the spotted sting or eagle ray, *Actobatus narinari*, while the others are probably from the black sting-ray, or "stingaree," *Dasyatis say*.

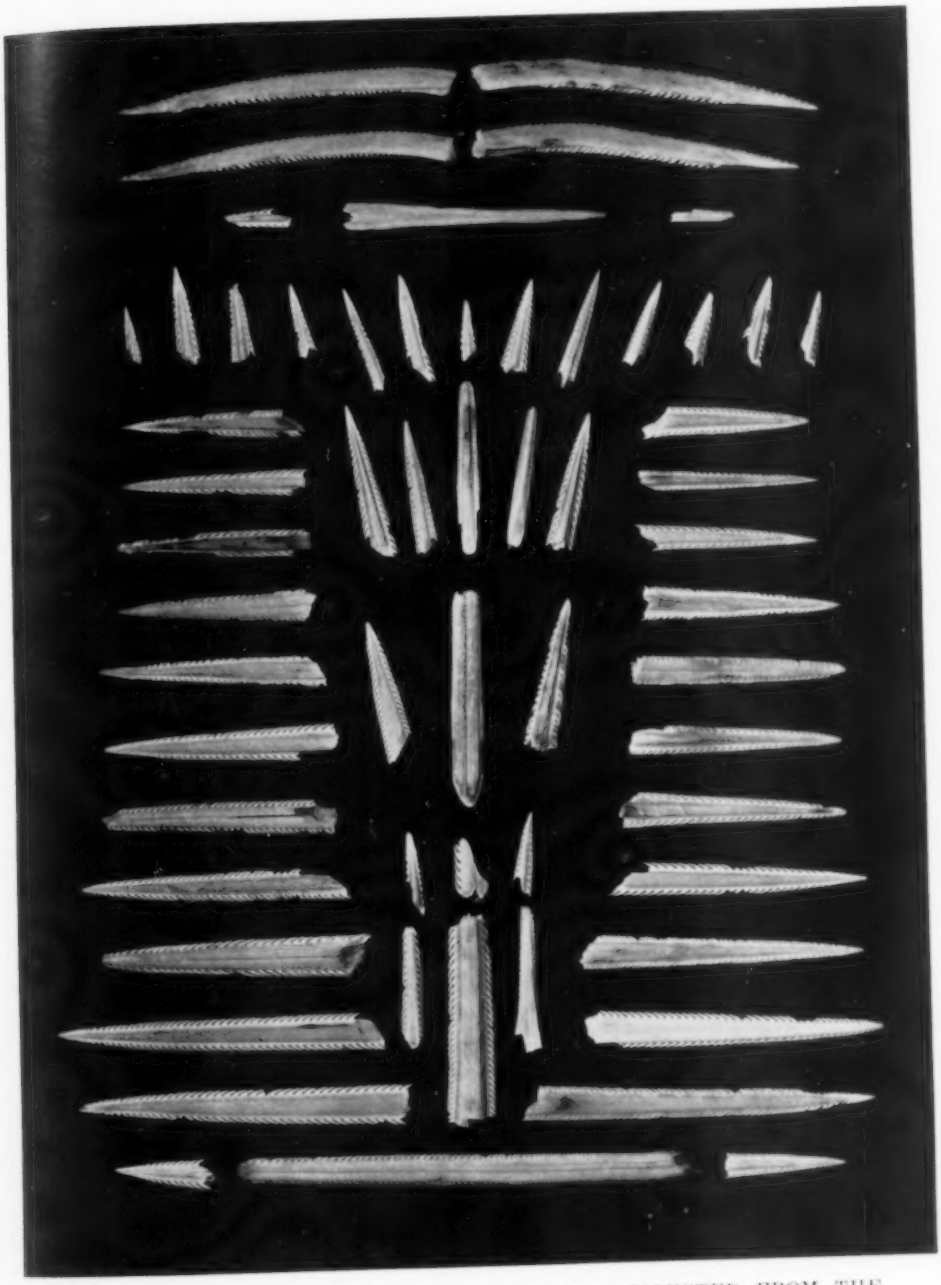


FIG. 2. A PANEL OF 58 STINGS OR SPINES DISSECTED FROM THE HAMMER-HEADED SHARK

OF THESE 54 ARE FROM THE STINGAREE AND THE SPOTTED EAGLE-RAY, AND AT THE TOP ARE 4 PECTORAL SPINES FROM THE OCEAN CATFISH. THESE WERE TAKEN FROM THE JAWS AND NEIGHBORING PARTS OF THE SHARK SHOWN IN FIG. 1.

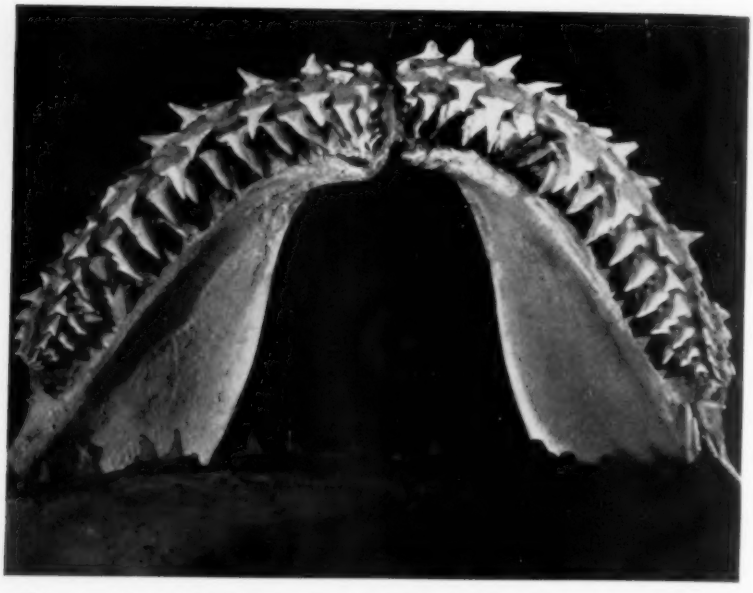


FIG. 3. THE LOWER JAW OF THE BLACK-TIPPED SHARK

THIS JAW OF *Carcharhinus limbatus* (SEEN FROM ABOVE) SHOWS A STING-RAY SPINE BETWEEN THE TWELFTH AND THIRTEENTH ROWS OF TEETH ON THE LEFT AND ANOTHER BETWEEN THE FIFTH AND SIXTH ROWS ON THE RIGHT. SPECIMEN IN THE COLLECTIONS OF THE AMERICAN MUSEUM.

the commonest sting-ray at Beaufort. The tail of the latter is used as a whip, and I have seen a large ray caught in a seine lash out savagely with its tail and drive its sting so deeply into the side of a boat that the ray could not free itself save by breaking off the sting. The large spine at the bottom of the figure was torn out by the root. However, it has its tip broken off. The same things are true of the short basal part of a spine just below the center of the figure.

These serrated stings will easily penetrate a leather shoe or a rubber boot and will cause very formidable and painful wounds very hard to heal. Fishermen and writers on fishes in all ages and climes have claimed that these wounds are poisonous. These stories even go back to classical antiquity, from which times fearful accounts of severe lacerated wounds have come down. Of them Francis Day writes in his "Fishes of Great Britain and Ireland" (p. 351, London, 1880) that:

> Pliny, Aelian, and Appian asserted that the venom with which it was endowed was capable of causing injury to even vegetable and mineral substances, trees losing their verdure, and rocks being even affected. Circe armed her son with a spear which she pointed with a Trygon's [a huge sting-ray's] spine as the most formidable weapon she could place in his hands, and with which he subsequently unintentionally slew his father, Ulysses. Anyhow, these wounds are dangerous, for at Cochin I have had to amputate the arm of a native which was mortified owing to a wound from one of these fishes having divided the muscles and other structures almost to the bone.

These accounts, like most that have come to me from fishermen, are greatly exaggerated, but the essential facts are incontestable. I long thought that the effects following a stinging by a stingaree were due to infections from bacteria carried in with the slime covering the spines. But investigation showed that the dreadful pain and swelling following immediately on the infliction of the wound were too rapid and severe to be explained by bacterial infection. So

then I fell back on the explanation that the almost immediate effects were due to the slime acting as a chemical poison when injected into the circulation. Now, however, thanks to the researches of an English surgeon, Dr. H. M. Evans, we know that there is at the base of the spine of the sting-rays a small poison gland and that its secretion flows down the groove of the spine and by this is carried into the flesh and blood of the victim. Fortunately, however, this secretion is only moderately poisonous, compared with that secreted by the poisonous snakes and even by certain bony fishes—*e.g.*, by *Synancia* and its allies. Furthermore, even as some snakes are impervious to the poison of other snakes, so the hammer-head seems immune to the poison of its cousin, the sting-ray. Sharks seem also largely devoid of feeling. Certainly the specimen under consideration must have been so since it attacked and was "stung" by at least 54 stingarees and 4 catfish—these adventures evidently covering quite a space of time.

So far as I can find in the literature examined there are on record but two other published observations of this habit of the hammer-head in feeding on rays. The late Russell J. Coles, in his observations on "The Large Sharks of Cape Lookout, North Carolina" (*Copeia*, no. 69, p. 42, 1919), speaks of examining large and presumably old hammer-heads at Cape Lookout which "subsisted exclusively [?] on sting-rays, which, as far as observed, were *Dasyatis*."

While reading the proof of the above, I received a most unexpected and interesting confirmation of this ray-eating habit of the hammer-head. Mr. Henry W. Fowler in describing certain fishes from Florida (*Proceedings Academy Natural Sciences Philadelphia*, vol. 78, p. 249, 1926) writes thus of a female hammer-head 13.2 feet long taken at Captiva Pass by Mr. R. D. Benson, Jr., dissected by him and the following data supplied:

> In the stomach . . . were 3 rays (evidently *Dasyatis sabina*) with a disc length of 18 to 24 inches, and in the mouth 1 ray of 20 inches. There were also remnants of 17 caudal spines in the stomach, the largest complete 3 inches and the largest estimated at 4 inches. The jaws were also punctured with other spines and at least 24 spines in the gum.

Had the jaws, neck and chest region been carefully dissected by Mr. Benson it is probable that he would have gotten an even greater number of sting-ray stings than I did. It is a great pity that he did not preserve these jaws with the stings in place. Mr. Fowler has sent me 12 of these stings, which I find counterparts of mine.

Earlier than he, however, Francis Day, in his "Fishes of Great Britain and Ireland" (Vol. II, p. 294, 1884), had recorded the fact that in the stomach of a 13-foot, 7-inch hammer-head taken in 1865 near Ilfracombe, on Bristol Channel, two thornback rays had been found.

From the very definite data given above it can not be doubted that large hammer-headed sharks feed largely on sting-rays. However, it has long been held that in general the hammer-head and the closely related bonnet-head (*Sphyrna tiburo*) seem to be bottom feeders and various writers have reported their stomach contents to consist sparingly of squids and largely of crabs, shrimps and other crustaceans; and of such fish as go in schools, menhaden and mackerel, etc. Coles reports mackerel and thinks it the chief food of the hammer-head.

The Black-Tipped Shark a Feeder on Sting-Rays

In the collections of the American Museum are a considerable number of sharks' jaws labeled as sent in from the Gulf of Aden, an arm of the western Indian Ocean connecting it with the Red Sea. These were presented by the late

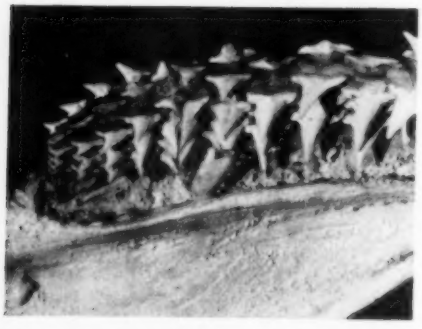


FIG. 4. A SECTION OF THE JAW OF THE BLACK-TIPPED SHARK

THIS PART OF THE LOWER LEFT JAW SHOWN IN FIG. 3 IS TO DEMONSTRATE THE STINGAREE SPINE EMBEDDED IN THE CARTILAGE OF THE JAW. IT HAS DIVIDED THE BUD OF THE THIRTEENTH ROW OF TEETH INTO HALVES AS IS SHOWN BY THE HALVED TEETH. SPECIMEN IN COLLECTIONS OF THE AMERICAN MUSEUM.

Dr. Alfred Ehrenreich, then connected with a French company catching sharks to make leather of their hides. Unfortunately no definite localities in this gulf are designated, but Captain W. E. Young, who was then connected with this fishery, informs me that practically all the fishing was done at Djibuti, French Somaliland.

This pair of jaws, which my colleague, Mr. John T. Nichols, has kindly identified as coming from that shark whose scientific name is *Carcharhinus limbatus*, had only been "roughed out." There was considerable muscular tissue attached to both inner and outer surfaces, and the folds of tough tissues, forming the "gums" or thecal folds and covering the reserve teeth, were intact but dry and almost as hard as bone. However, there could be seen between the twelfth and thirteenth rows of teeth in the lower left jaw the broken-off basal part of a sting-ray sting embedded in the gum and cartilage. The point of this protruded on the outside of the jaw cartilage slightly to the left of a vertical line drawn through the middle of this jaw.

This was enough to start me on the rather difficult task of a thorough dissection of these dried jaws. This, when finished, brought to light the following interesting state of affairs. The spine referred to had pierced the tooth bud of the thirteenth lower left row of teeth, resulting in the formation of *two* rows of teeth rather smaller than the normal-sized teeth. This whole lower jaw is shown in Fig. 3, while Fig. 4 is a photograph showing in larger size the broken-off basal part of the enlarged spine. Fig. 5 shows the point of the spine where it has emerged on the outside of the jaw cartilage. From these figures some idea can be gotten of the tremendous power with which the ray lashes out with its tail.

Furthermore, on the lower right jaw just outside of the fifth row of teeth, was found a shorter and smaller spine with its point embedded in the tooth bud tissues at the base of the hollow of the jaw. This spine, in place as found, is shown in Fig. 3. However, as may be seen in Fig. 6 (where it has been removed and cemented to the inner edge of the jaw cartilage), it merely clipped off a small portion of the tooth bud, which has successively given rise to five small fragments set to the right of this fifth row of teeth. The details of these two remarkable teratological rows of teeth will be discussed in another paper.

Further dissection brought to light two other fragments of sting-ray spines embedded in the gums. These have been removed and affixed to the left inner segment of the lower jaw. They are the two larger spines shown at the extreme left in Fig. 6. Three other spines were found stuck in the membrane covering the lower left jaw. Above the two spines referred to was seen on the rim of the jaw cartilage a very small point of a spine. This, being invisible as the jaws were photographed, was removed and cemented to the jaw to the right of the two large spines, as may be seen in Fig.

6. To the left of the two larger points are two small points (one almost too small to show in the photograph) *in situ*.

Dissection of the right angle of the mouth brought to light a similar state of affairs, five spines being concerned. As may be seen in Fig. 6, two tips were found on the inner side of the broad base of the jaw; while on the edge of the jaw cartilage just beyond and to the right of the end of the tooth band are three others. The larger one is broken in three pieces. At its base are two small points of stings found with their apices pointing outward and somewhat toward the symphysis of the jaw. In this connection it should be noted that the small fragment on top of the lower left jaw (referred to in the preceding paragraph) pointed squarely toward the symphysis. It is very difficult to see how these three tips could have been embedded to point as they are found to do.

Just here it may be emphasized that all these spines are in their natural positions, as shown in Fig. 6, save four. The two large stings on the lower left jaw were found pointing in their present direction but set in the gum of the jaw. If put back there they would be difficult to affix and would be largely invisible. The third spine on the edge of the right jaw, about one third to the left of the symphysis, has been taken from its place and affixed where it is shown in order to make visible the divided tooth row—the fifth to the left. The fourth spine is the small point shown to the right of the two larger spines on the left lower jaw. Still another point was found on top of the upper jaw just to the left of the symphysis. It has unfortunately gotten lost.

A total of 13 spines were found in the roughly dissected jaws as they came to my hand. Presumably, and one may almost say undoubtedly, if the muscles attached to the jaws and indeed if the whole throat and chest region could have been dissected, a considerably larger number of stings would have been found. It is plain that this shark had for a long time been feeding on sting-rays. The two larger spines prove this. The one penetrating the tooth bud of the thirteenth row of the lower left jaw, and piercing the very cartilage of the jaw, had protruded on the exterior and in doing so had divided the tooth gum into halves. The other sting had severed the tooth gum into two parts, one large and the other very small. Each interference with a tooth bud had occurred so long ago that the old undamaged teeth have all rotated upward and have wholly disappeared and their places have been taken by the abnormal teeth formed since the injury to the tooth buds. This phenomenon will be referred to later.

The Short-nosed Shark a Ray-eater

The evidence that the short-nosed shark, *Hypoprion brevirostris*, is an eater of sting-rays rests upon the presence of the tip of one sting found adher-

FIG. 5. THE PROTRUDING TIP OF THE STING

The tip of the sting shown in Fig. 4 has penetrated the whole cartilage of the jaw and has emerged on the outside. This effect could only have been produced by a very strong lashing out of the tail. Specimen in collections of the American Museum.

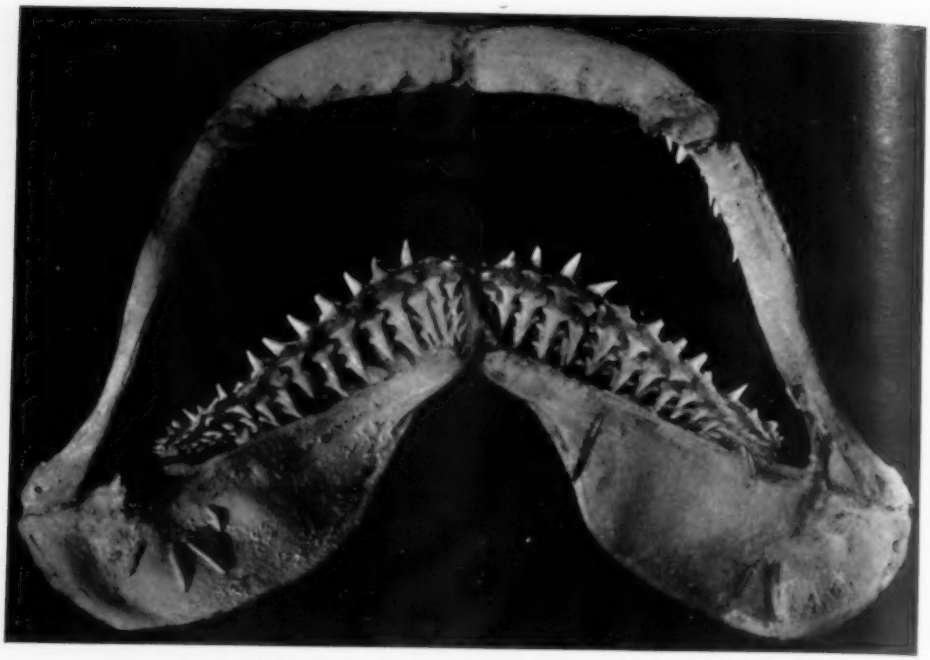


FIG. 6. THE LOWER JAW OF THE BLACK-TIPPED SHARK

THIS IS PHOTOGRAPHED FROM BEHIND AND ABOVE. NOTE THE FRAGMENTS OF VARIOUS STINGAREE SPINES, 12 IN ALL—(11) ADHERENT TO AND (1) EMBEDDED IN THE JAW. SPECIMEN IN THE COLLECTIONS OF THE AMERICAN MUSEUM.

ent to one jaw of a shark of this species. The jaws under study were dissected from a female specimen taken at Sanibel Island, off the west coast of Florida, by Dr. R. C. Murphy, in January, 1919.

On the left lower jaw, about one third of the distance from the symphysis to the angle of the jaw, is the tip of a sting measuring 10 mm long. This is set to point toward the symphysis but downward at an angle of about 45 degrees. This sting was undoubtedly thrust into the tissues covering the jaw when the ray was caught by the shark. In the case of this specimen, as in those described above, the conjecture is allowable that if the dissection of the jaws had been carried on with a view to finding stings, more of these would certainly have been discovered.

THE TIGER SHARK AN EATER OF RAYS

This habit of the tiger shark (*Galeocerdo tigrinus*) has incidentally been known since 1784, when William Andre published an article and a figure in the *Philosophical Transactions of the Royal Society of London*. The last section of this article and the last of his figures were together intended to prove what has been incidentally shown in the last paragraph above—"That the Teeth of Cartilaginous Fishes are Perpetually Renewed." We will let Andre speak for himself.

> Being engaged in dissecting the jaws of a very large shark, I was surprised to find a portion of that sharp, bearded bone found in the tail of the fire-flaire, or sting-ray, driven quite through the lower jaw among the posterior teeth, and fixed almost immoveably. How this happened must be obvious to every one [Fig. 7]. . .

The sharp bone of the sting-ray was fixed in the lower jaw between two rows of teeth, and at their posterior part, where the first rudiments of the future teeth are formed, and it will be clear to every one, particularly those who are conversant in such matters, that this could not have happened without producing a great deal of pain, swelling and disorder in the part where it was fixed. It is unnecessary to enumerate the different kinds of mischief this might occasion. Let it suffice to observe, that on account of the space taken up by this extraneous body, the teeth on each side of it, for want of room, could never after be perfectly formed. The teeth on the left-side wanting their angles to the right, and the teeth on the right-side being destitute of their angles to the left.

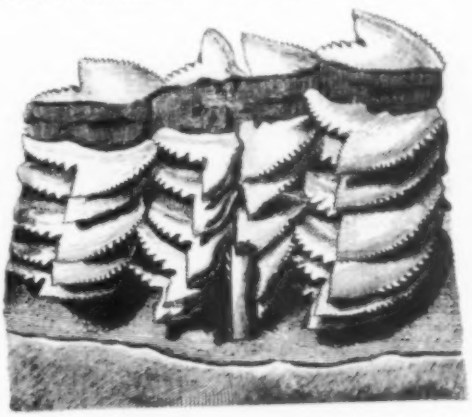
As it is certain, that the anterior teeth were formerly posterior ones, and as the teeth in each row were all deficient in one angle, it follows, that they must have been formed posterior to the insertion of this extraneous body. Again, if we allow that before the accident the animal was in possession of perfect teeth, it follows also, that they were consumed and replaced by imperfect ones.

Of course, in the light of what has been shown as to the multiple attempts of both the hammer-head and the spotted-fin to catch and feed on sting-rays, our author is in error as to the "pain, swelling, and disorder" resulting from the embedding of the sting-ray spine in the jaw of the tiger shark. Nor does he seem to have understood that the tooth bud was divided by the sting so that from the parts have arisen two imperfect right and left halves of what would, but for the sting, have been a normal row of teeth. This is plainly shown in his figure (Fig. 7 herein).

But he does explain how the disturbance caused by the embedding of the "sting" has somehow caused the formation of the divided teeth. He further notes that the normal teeth above the spine have all been replaced and their places taken by abnormal teeth; that since 6 normal teeth have been replaced by 6 abnormal ones, this must have taken place a long time ago. Then he adds, "I have endeavoured to prove that a part of the inhabitants of the great deep retain, in the article of teeth, a perpetual juvenility, being apparently utter strangers to edentulous old age."

Commenting on the succession of teeth in sharks, Richard Owen, in his "Odontography" (p. 39, London, 1840), says that to determine the matter it would be necessary that ". . . a foreign body, *e.g.*, should be inserted into the base of the jaw of the shark, and a tooth in the corresponding place should be so marked, as at a subsequent period it might be recognized, and its position compared with the perforated part of the jaw." Then he rather naïvely adds that "Such an experiment would not be very practicable in the carnivorous inhabitants of the deep we are now considering, but accident has satisfactorily supplied its place." Then Owen goes on to reproduce Andre's figure and account referred to above, without, however, giving Andre any credit.

I have captured a considerable number of tiger sharks and have dissected their jaws, and there is in the American Museum a large collection of such jaws, but none of these contain stings. Dissection of the stomachs of a number of



—*After Andre, 1784*

FIG. 7. AN EMBEDDED STING-RAY'S SPINE

THIS STING HAS BEEN EMBEDDED IN THE JAW OF A TIGER SHARK (*Galeocerdo tigrinus*) SO LONG THAT THE NORMAL TEETH HAVE DISAPPEARED AND THEIR PLACES HAVE BEEN TAKEN BY ABNORMAL ONES.

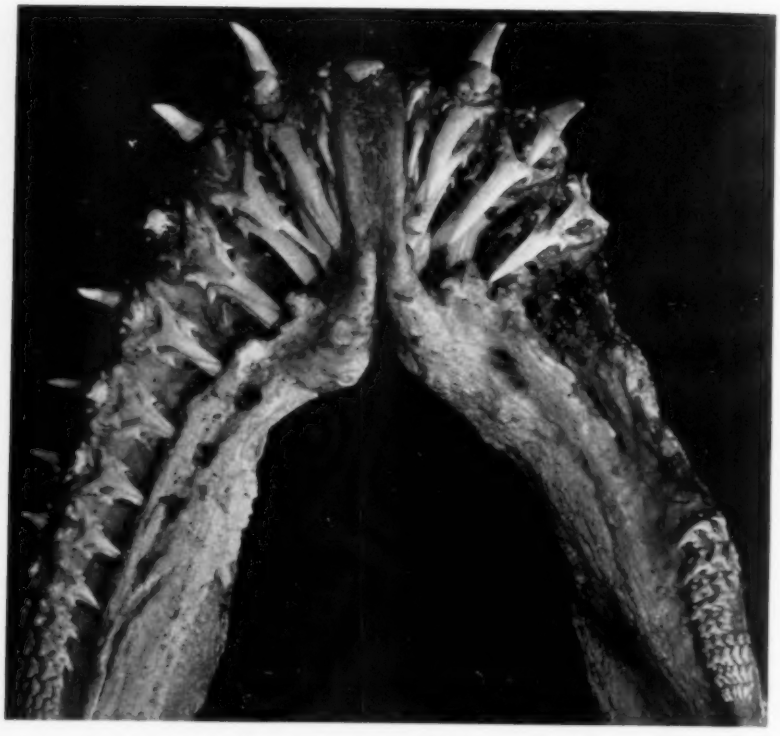


FIG. 8. LOWER JAW OF A MACKEREL SHARK

THIS FIGURE MADE FROM ABOVE SHOWS THE RIGHT LOWER JAW OF *Lamna* SP. TO HAVE SUFFERED GREAT INJURY. THE TEETH AND TOOTH BUDS OF ROWS 5 TO 9 INCLUSIVE HAVE BEEN COMPLETELY EXTIRPATED. SPECIMEN IN THE COLLECTIONS OF THE AMERICAN MUSEUM.

tigers (some caught in Florida with sting-ray bait) has brought to light an extraordinary variety of foodstuffs, but no remains of sting-rays. Furthermore, in all my tiger shark fishing at Key West I have never seen a tiger shark trying to catch a sting-ray. However, at least one other man has found the indubitable evidence of such cannibalism.

In July and August, 1920, Mr. J. C. Bell, of the Museum's department of preparation, spent some weeks at Morehead City, North Carolina, as guest of the Ocean Leather Company. His particular errand was to make for the American Museum plaster moulds of sharks and rays, as these fishes were brought in from the net fishery at Cape Lookout. Fortunately, however, Mr. Bell made notes on stomach contents also, and these were later worked up and published by him and Mr. J. T. Nichols. Their article "Notes on the Food of Carolina Sharks" (*Copeia*, No. 92, pp. 17–20, 1921) is of very great value. So also is Mr. Nichols' more popular article, "What Sharks Really Eat," published in *Natural History* (Vol. 21, pp. 272–278, figs., 1921), based in large part on the same data.

Bell and Nichols record three tiger sharks, varying from 10.5 to 11.5 feet in length, taken on August 13. Two of these contained large pieces of the smaller devilfish, *Mobula olfersi*, and the third a part of a whip-tailed ray. Five days later, a large tiger shark was found to contain a large sting-ray cut into

three pieces. Another had bitten off and swallowed the head of a *Mobula*. The fact that these sharks had dined on the rays may possibly be due to the fact that they were all cooped up in large nets which took some time to clear, thus giving the sharks time to prey on the rays.

Captain W. E. Young, who has a wide knowledge of these denizens of the deep, has kindly given me certain general accounts of sharks feeding on rays which will now be incorporated herein. He states that:

> Sharks everywhere seem fond of rays and may be found feeding on them. These rays are all provided with from one to five sharp stingers of bone with barbs pointing backward. These stingers inflict terrible wounds on human beings, but though the ray stings the shark when it catches and eats the ray, the stings do not seem to injure the shark. The shark cuts up the ray, and swallows these parts, the tail often being included. Thus the stingers get into the stomach of the shark where I have often found them. Eventually these work out through the wall of the stomach and get into the muscles and work toward the head or the tail according as they are pointed. I have found a sting on the back under the skin and another up near the brain.

At St. Thomas he witnessed an attack by a shark on a ray, which he describes as follows: "The shark found the ray in the corner of the boat landing. After sparring for an opening the shark darted in, caught the ray in its mouth, and made off into the deep water. The ray put up a good fight but its sting was no match for the teeth of the shark."

He also states that while at Anegada, British West Indies, a ray was caught in one of his nets. It was left therein for some time, whereupon a tiger shark endeavored to eat it and was itself entangled in the net.

The largest of the rays is the gigantic *Manta*, which may reach the extreme width of 25 feet, but which is perfectly harmless and, lacking a spine, defenseless. Captain Young caught many of these at Warimos Island in Djibuti Harbor, French Somaliland. A considerable number of these mantas had had their fins bitten by sharks. However, the *Manta* is such a gigantic ray that even a large ray could hardly do more than take some bites out of its great pectoral fins.

SHARKS PREYING ON OTHER SHARKS

It is generally known that if a shark is caught on a hook, hauled on board a

FIG. 9. THE RIGHT LOWER JAW OF A MACKEREL SHARK

THIS SHOWS THAT A WHOLE SECTION OF THE CARTILAGE OF THE JAW HAS BEEN TORN OUT, BUT HAS BEEN LARGELY REPLACED THROUGH REGENERATION. SPECIMEN IN COLLECTIONS OF THE AMERICAN MUSEUM.

FIG. 10. A LARGE TIGER SHARK

THIS SPECIMEN OF *Galeocerdo tigrinus* IS SWUNG BY THE TAIL TO THE CRANE ON THE WHARF AT THE LABORATORY OF THE CARNEGIE INSTITUTION OF WASHINGTON AT LOGGERHEAD KEY IN THE DRY TORTUGAS, FLORIDA. NOTE THE PARTLY EVERTED GULLET DISCHARGING THE FEATHERS OF A WATER BIRD.

boat, chopped up with an axe and thrown overboard, it will be devoured by other sharks. Also it is a matter of common knowledge that, around a whale being "cut in" if one of the sharks scrambling for tidbits is disabled by a whaleman's spade, it will be speedily devoured by its companions.

A vivid general account of shark-eat-shark has recently been given by Dr. C. H. Townsend, director of the New York Aquarium, in the *Bulletin of the New York Zoological Society* (Vol. 34, p. 163, 1931). While on the *Albatross* off the Bahamas some years ago, Townsend saw the vessel surrounded by a great many sharks. Nearly a dozen were captured, cut open and thrown back into the sea. "The sharks, attracted by the bloody carcasses gradually being thrown overboard, provided an exhibition of how hungry sharks behave in the presence of an unexpected supply of food [in the shape of dead sharks]."

There is in the collections of the American Museum a badly mangled jaw of a mackerel shark (*Lamna sp.*) of unknown source. As may be seen in Figs. 8 and 9 the right lower jaw has been seized (no other marine animal than a shark with like strong front teeth could have done this), and the jaw with the tooth buds Nos. 5, 6, 7, 8 and 9 has been torn out. The cartilage of the jaw has by regeneration filled in, from sides and bottom, the torn-out cartilage of the jaw, but the tooth buds totally extirpated make impossible any regeneration of the missing teeth.

However, definite accounts by scientific men of this feeding by definitely named sharks are few and far between. I have found but four, and these accounts will now be set out under the name of the shark that we may have definite knowledge of cannibalism by certain particular sharks.

The Tiger Shark a Cannibal

This shark (whose scientific name, as we have seen, is *Galeocerdo tigrinus*) is definitely known to be a cannibal. Our first direct knowledge that this shark will feed on its fellows we owe to Lewis Radcliffe. In his "Sharks and Rays of Beaufort, North Carolina" (*Bulletin U. S. Bureau of Fisheries*, Vol. 34, p. 263, 1916), there is a very realistic account of this cannibalism. A tiger, 10 feet, 1 inch in length (one of three already captured) has been suspended from a boom with its head out of water. Subsequent events Radcliffe describes as follows:

> About this time a shark, larger than any of those taken, swam up to the one hanging from the boom, and raising its head partly out of the water, seized the dead shark by the throat. As it did so, the captain of the *Fish Hawk* began shooting at it, with a 32-caliber revolver, as rapidly as he could take aim. The shots seemed only to infuriate the shark, and it shook the dead one so viciously as to make it seem doubtful whether the boom would withstand its onslaught. Finally it tore a very large section of the unfortunate's belly, tearing out and devouring the whole liver, leaving a gaping hole across the entire width of the body large enough to permit a small child to easily enter the body cavity. At this instant one of the bullets struck a vital spot, and after a lively struggle on the part of the launch's crew, a rope was secured around its tail. The four specimens, all females, were brought to the laboratory for examination. The last shark was 365.8 cm (12 feet) in length, and the liver of the smaller one was still in its stomach, the estimated weight of which was 40 pounds. At the time of capture one of the sharks regurgitated a rat, another a small shark about 61 cm in length.

Of the tiger shark, Coles writes (*Copeia*, 1919) that in June–July, 1918, he found in the stomachs of three small tiger sharks at Cape Lookout, North Carolina, "cleanly bitten pieces of freshly eaten shark meat with the skin attached, just as if the chunk of meat had been cut from the side of a shark." Again on June 25, from a 7-foot, 9-inch specimen, he obtained "eleven chunks of shark meat of from one to five pounds in weight." These he says were torn from hammer-headed, sharp-nosed and ground sharks. That these pieces were shark flesh was of course easily ascertained, but only microscopical examina-

FIG. 11. THE JAWS OF A LARGE TIGER SHARK

FOR COMPARISON A 12-YEAR-OLD BOY HOLDS AND LOOKS THROUGH THESE. NOTE THE HUGE GAPE AND THE LARGE SICKLE-SHAPED CHOPPING TEETH.

tion of the denticles would give diagnoses of genera and species.

From the same locality, in July–August, 1920, Bell and Nichols (*Copeia*, pp. 18–19, 1921), report stomach contents of tiger sharks as follows. On July 9, a *Galeocerdo*, 11 feet, 3 inches long, contained "a large piece from the side of the head and gills of an 11-foot hammer-head taken in the same net with it." On July 13, the stomachs of four tigers contained large pieces (some of them equal to half of the shark) of the black-tipped shark (*Carcharhinus limbatus*). Two 10-footers, on July 16, contained three or four young hammerheads. On July 31, a 12-foot, 2-inch tiger (weight 630 pounds) "contained a large shark [kind undetermined] of about eight or nine feet bitten in seven or eight pieces." An 11-foot male (450 pounds) captured on August 5, "contained a small hammer-head of about 3.5 feet intact, and several parts of other sharks." On the same day, two females of 9 feet, 10 inches, and 11 feet, 6 inches, contained small pieces of shark, probably a black-tip.[1]

Captain Young informs me that while net-fishing for sharks at Port Stephens, New South Wales, Australia, he got a large tiger shark with the belly and sides badly torn. When the net was cleared the last shark was a tiger. When this was made fast by the tail and hauled up by the derrick on the wharf, it disgorged through the mouth a great mass of skin, fin, liver and intestines. All these were easily identified as having come from the first tiger captured. Again near Fernandina, Florida, a female tiger shark was caught, and, when hauled up by the tail, disgorged three large sections of tiger shark meat (easily identified by the stripes) and a lot of feathers from a water-fowl.

The gullet of the tiger shark, like that of many other sharks, is merely a large passageway connecting the huge mouth and the capacious stomach. When the shark is hung up by the tail the gullet tends to become everted and the stomach evacuates through this any large undigested material. This may be seen in Fig. 10, which shows such a tiger shark which I captured at the Dry Tortugas, Florida, years ago.

CANNIBALISM AMONG HAMMER-HEADS

The hammer-head (*Sphyrna zygaena* by name), portrayed in Fig. 1, and in the preceding section shown to be a victim of the tiger shark, is also a straight cannibal. This was first reported by Coles (1910), who writes as follows of

[1] Much of this data may also be found in Nichols' article in *Natural History*, Vol. 21, pp. 272–278, 1921.

a 13-foot, 10-inch female specimen captured at Cape Lookout in July, 1918:

> At the time of her capture, she had just eaten four of her own species from my net, two of which had been swallowed whole, except the heads of the five-foot examples, and there were four cleanly cut pieces, which represented entire bodies except heads, of two or more hammer-head sharks; then the stomach contained more than a peck of vertebrae of sharks, provisionally identified [?] as her own species.

The Cub Shark also a Cannibal

For this shark (*Carcharhinus commersonii*) cannibalistic habits similar to those in the tiger are reported by Bell and Nichols (1921). On July 13, 1920, two were taken which had fed on smaller black-tipped sharks (*Carcharhinus limbatus*). None others, however, contained shark flesh, the food mainly being various bony fishes.

The Mackerel Shark a Shark-eater

I have on pages 412, 413 (Figs. 8 and 9) and 415 figured and described a jaw of this shark which had been torn by another shark. That the European form, *Lamna cornubica*, a fierce and ravenous shark, also feeds on other sharks is attested by Couch (1867, p. 43) who notes that he found in the stomachs of various specimens the skeletal remains of cartilaginous fishes. And Day (1884, p. 298) expressly states that in the stomach of an 8-foot female taken at Mevagissey, southern Cornish coast, in 1881, there was found a large partially digested dogfish.

Miscellaneous Accounts of Cannibal Sharks

A careful search through the literature would undoubtedly bring to light many miscellaneous notes on this subject. I have chanced upon three which may be added for completeness' sake. Couch (1867, p. 29) states that from the stomach of a blue shark (*Prionace glauca*) he once took a large piked dogfish bitten across the middle into two pieces. Of the spinous shark, *Echinorhinus spinosus*, John Gatcombe states (*Zoologist*, Vol. 35, p. 108, 1877), that one captured off Plymouth, England, in that year contained four piked dogfish. And Day (p. 324, 1880), carries the story further by recording that a specimen of the spinous shark, taken off the Eddystone Light in 1869, had in its stomach "several dog-fishes, some 3 feet in length." Since these dogfishes are a great nuisance to the commercial fisherman, these enemies of theirs must be credited as friends of the fishermen.

Cannibalistic Practices by Unidentified Sharks

Heretofore the data concerning these practices among elasmobranchs has been set out under the name of the aggressor. That now to be presented concerns cases in which the victim in some cases is known, but in none is the predator known.

—Photograph by courtesy of Capt. W. E. Young

FIG. 12. THE RESULTS OF CANNIBALISM

THIS TIGER SHARK WAS CAUGHT IN A NET AND WHILE HELD THERE WAS PARTIALLY DEVOURED BY OTHER SHARKS.

—*Photograph by courtesy of Capt. W. E. Young*

FIG. 13. A VICTIM OF CANNIBAL SHARKS

THIS HAMMER-HEADED SHARK (*Sphyrna zygaena*) FIGURED HERE WAS CAUGHT IN A NET AND BADLY TORN BY OTHER SHARKS.

My sole experience of shark-eat-shark is as follows. While at the Tortugas Laboratory of the Carnegie Institution of Washington in 1914, on the afternoon of July 14, I set a shark hook buoyed to an empty five-gallon oil-can off our dock on the west side of Loggerhead Key. Shortly after daylight the next morning the can was seen to be violently agitated and the back and tail fin of a large but unidentified shark were observed near it. Since the cook's wife was with him and since the mess house was between the dormitory and the dock, it was impossible to go for the shark in ordinary Tortugas costume. But as soon as I could throw on some clothes I hurried out to the hook and found the head and neck parts of a short-nosed shark, *Hypoprion brevirostris*, everything from the pectoral fins backward being *gone*. An inquest was held over the remains and the shark was identified as above. The shark which had been seen had evidently eaten his fellow. The commotion in the water had plainly been caused by the attack of the one shark and the efforts to escape of the other. The hooked shark was at a terrible disadvantage and could do nothing but flounder about. Unfortunately, the marauder was never taken.

Captain Young had a similar experience once. On overrunning his hooks at Anegada, British West Indies, one morning he found on one the head only of a dusky shark, which he estimated would have weighed about 250 pounds. The whole body from tail to gills had been eaten by other sharks.

My own experiences of shark-eat-shark cannibalism are unfortunately confined to the case above set out, but from the extensive notes made by Captain Young I am privileged to extract the following general data and make them available for the reader. The first case illustrates the voracity of the shark and its insensibility to pain.

At Water Island, one of the Virgin group, West Indies, in 1928, a tiger shark was hooked, but after a hard fight broke away with a torn jaw. The next morning this same shark with the torn jaw was found on one of the hooks badly eaten by other sharks. This shark is shown in Fig. 12, where it is seen that the whole side had been torn away from gills to the end of the abdominal cavity and practically all the internal organs had been eaten, also the pectoral fin had been torn away.

A hammer-head was taken in a net at Frenchman's Cape, Virgin Islands, in 1928. Caught in the net almost always by the peculiar hammer-shaped head, the remainder of the body is generally clear and especially tempting for attack by other sharks. How this shark was "mauled" by other sharks may be seen in Fig. 13. Note that it is the

abdomen which is always attacked first. This is softer, and allows the marauder to get a firmer bite than any other part of the victim. However, other parts may be taken if competition is great. Thus Captain Young found in a shark's stomach opened at Morehead City, North Carolina, the whole tail of a large shark.

These records plainly justify the accusation of cannibalism against sharks, but in some cases this cannibalism was made easy by the fact that all these sharks (save Radcliffe's) were cooped up in the narrow confines of a net. In the open the tigers and the other sharks would have had difficulty in catching these other sharks. In the net either voracity or panic, or both, would lead to the attacks with the results noted. These bitings are facilitated for the tiger by the fact that it particularly has a mouth of huge gape beset with many large, sharp, sickle-shaped, chopping teeth. Such a jaw is shown in Fig. 11.

CANNIBALISM AMONG RAYS

This little section must be added, for unexpectedly and almost unbelievably accounts have been found which justify this addendum. I say "almost unbelievably" because ordinary rays have relatively very small mouths filled with comparatively small and non-biting teeth used for crushing crustacean and other relatively soft-bodied food. However, in the pavement-toothed rays, their mill-like teeth are used for crushing the shells of the oysters and clams which constitute their chief food. That rays should prey on other rays seems preposterous. But here follow the statements of some of the most distinguished English ichthyologists.

Raia batis a Cannibal

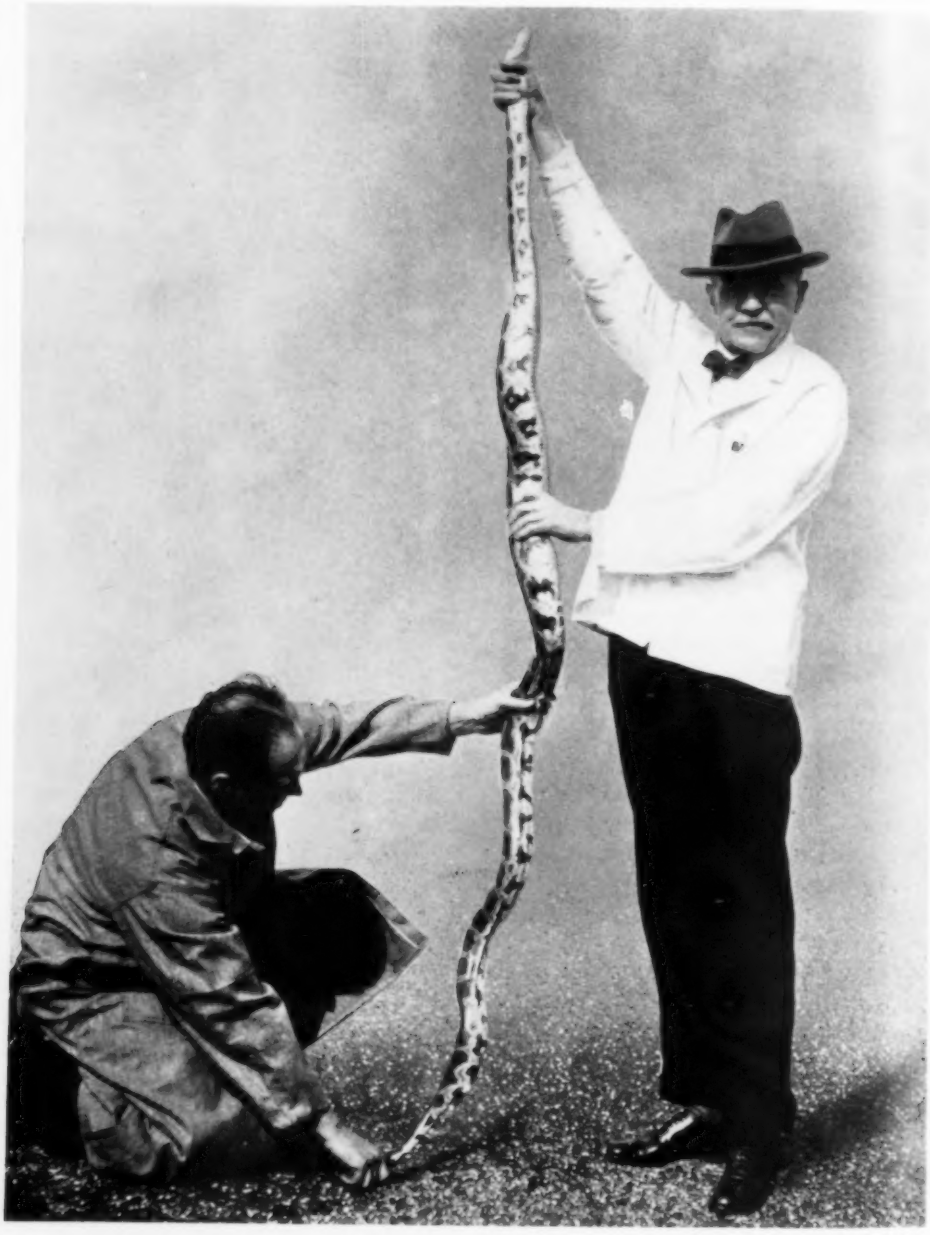
The common English skate, *Raia batis*, is the only ray, so far as I know, to have such cannibalistic habits attributed to it. Of this ray Francis Day, in his "Fishes of Great Britain and Ireland" (Vol. 1, p. 337, London, 1880), says that a specimen five feet wide was exhibited at Gravesend in December, 1881, which had in it a small skate and a dog-fish two feet long together with a lobster. He apparently was an eye-witness in this case.

Of this same habit in this same ray, Jonathan Couch says in his "History of the Fishes of the British Islands" (p. 88, London, 1867) that he dissected several and that in the stomach of one (size not given) he found a "thornback ray about eighteen inches in length."

Lastly, of this same ray, we read in Wm. Thompson's "Natural History of Ireland" (p. 259, London, 1856) of the capture on the north coast of Ireland of a huge specimen 5 feet, 8 inches wide; 7 feet, 3 inches long; and weighing 224 pounds. Its capture was effected thus: "A small skate got meshed [in a net] and was swallowed with a piece of the trammel of the net by the large one; and [the latter] being thus entangled [and held by the swallowed net] . . . was easily secured by the fishermen."

Thus then ends the tale, so far as the data are at hand, of cannibalism among the elasmobranchs. Fuller investigation of the records would undoubtedly bring to light other cases. However, I have taken my own observations and have added to them those which have readily come to my hand.

With regard to sharks feeding on rays, it is too much to say that these specialized elasmobranchs constitute the steady diet of hammer-heads, black-tips and tigers, but it is certain that they constitute the *pièce de resistance* of the two particular sharks first studied, as is plainly indicated by the number of sting-ray stings found in their jaws and attached tissues. For the other sharks, it is clear that under favorable conditions (restricted space especially) they are plainly cannibalistic, and are far more dangerous to their kinsmen than to the genus *Homo*.



A LIVE EIGHT-FOOT PYTHON, WEIGHING TWELVE POUNDS, BEING HELD BY DR. FRANCIS G. BENEDICT

MANY ANIMALS, BOTH WARM- AND COLD-BLOODED, HAVE BEEN STUDIED IN THE NUTRITION LABORATORY OF CARNEGIE INSTITUTION IN THE EFFORT TO INTERPRET PHENOMENA IN HUMAN PHYSIOLOGY, PARTICULARLY PHENOMENA RELATING TO HEAT PRODUCTION, HEAT LOSS, HEAT REGULATION, AND BODY TEMPERATURE.

REPTILES USED IN STUDY OF HUMAN PHYSIOLOGY[1]

By Dr. FRANCIS G. BENEDICT

DIRECTOR, NUTRITION LABORATORY OF CARNEGIE INSTITUTION OF WASHINGTON

SINCE the day when our first parents tasted the fruit of the Tree of Knowledge and the serpent was condemned to crawl on the ground on its belly, loathed by man and crushed under foot, the snake has been almost universally abhorred. This feeling of repugnance for the snake persists with many of us even to-day, in spite of the fact that this animal now serves many useful purposes. Thus, certain snakes such as the gopher snakes are of real economic importance in some localities because of their activity in destroying rodents, and snakes' skins are becoming more and more popular for trimmings on shoes and purses.

When we summon a physician to the house or consult him at his office, we do not stop to realize that many a lowly animal, such as the snake, has indirectly contributed its small share to the learning possessed by the physician. In order to diagnose our ailment correctly and prescribe treatment intelligently, the physician must have knowledge first of all concerning the physiological processes that take place in the body when we are well and healthy.

For this reason laboratories of research were established, where physiologists have been trying to discover what are the normal reactions and the normal functions of man. Each individual organ and each anatomical part of the human body has been studied under normal conditions, in the attempt to secure all the information possible pertaining to that particular function. The study of the diseased body, or the study of human pathology, has likewise been carried on as an indirect method of obtaining knowledge regarding normal human physiology. Thus, information regarding the abnormal liver of man in disease has proved of great value in securing more exact knowledge of the normal function of the liver.

Another indirect means of understanding the functions of the human body is the study of animal physiology. Many observations have been made with different animals, the object of most of which has been the ultimate perfection of knowledge regarding the physiology of our own bodies. In some animals the particular organ or function in which the medical man or scientist may be interested is especially elaborated. The digestive organs of the ruminant, for instance, are so elaborate that the food remains in the alimentary tract for a much longer time than is the case with man. This longer retention of the food results in fermentations and chemical reactions, the study of which helps in interpreting certain digestive processes in man.

In studying animals to clarify the problems encountered with humans, scientists recognized years ago the value of including cold-blooded animals. It has been found that the best indications as to whether our body is functioning normally are our heat production and our body temperature. The amount of heat that we produce and our body temperature show whether our vital activity or our life processes are at a normal or an abnormal level. To aid in the inter-

[1] A monographic report covering the details of this investigation is now in press. It will appear under the title: "The Physiology of Large Reptiles, With Special Reference to the Heat Production of Snakes, Tortoises, Lizards and Alligators." It will be listed as Publication No. 425 of the Carnegie Institution monographic series. This article has been prepared by Miss Elsie A. Wilson, of the staff of the Nutrition Laboratory.

MEASURING THE LENGTH OF A SEVENTEEN-FOOT PYTHON WEIGHING SEVENTY POUNDS

PHOTOGRAPH TAKEN BY MR. E. R. SANBORN IN THE REPTILE HOUSE AT THE NEW YORK ZOOLOGICAL PARK.

pretation of the abnormally high temperature of man during fever it was early thought expedient by investigators to note the effect of rapidly changing body temperatures in the case of cold-blooded animals. The body temperature of the cold-blooded animal changes with the temperature of the air surrounding it.

How do these changes in the body temperature of the cold-blooded animal affect its body and how can a study of these changes aid in understanding the cause and results of fever in man? These are questions typical of those that are continually presenting themselves to the medical man and the research worker. Some of them have now been answered. Others remain yet to be solved. But there is no doubt that the study of animal physiology is necessary for the best understanding of human physiology.

In 1907 the Nutrition Laboratory was established in Boston by the Carnegie Institution of Washington, for the study of problems in human and animal nutrition. Since its establishment, the Nutritional Laboratory has undertaken an extensive program of research on heat production and thermal relationships, not only with humans but with a large number of other warm-blooded animals ranging in size from an 800-kg. horse to a 22-gram mouse. In the furtherance of this program the heat production of cold-blooded animals under various conditions has likewise been studied.

The first experiments carried out by the Nutrition Laboratory with cold-blooded animals began in 1915 at the New York Zoological Park and continued over a period of 17 years. Nearly one thousand observations were made, chiefly with snakes but also with a number of lizards, alligators and giant tortoises. Thirty-six snakes were used which varied in size from a 5-pound gopher snake to a 17-foot reticulated python of 70 pounds.

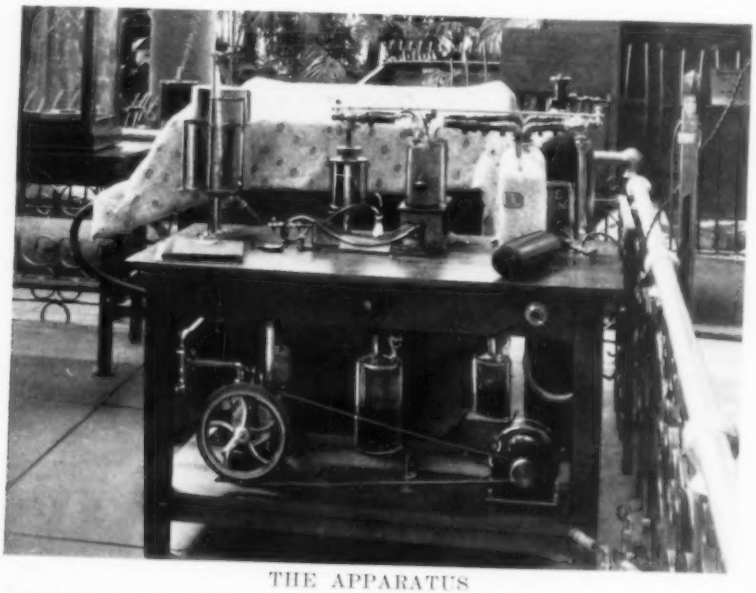
The accompanying photographs give an approximate idea of the size of some of the larger snakes. The 17-foot python had to be held by five men in order to obtain measurements of its length and body girths. A 9-pound and 120-pound alligator, 8 lizards, weighing

about 2 pounds each, and 9 tortoises, weighing from 10 to 300 pounds, were also studied. Most of these animals were obtained at the New York Zoological Park through the courtesy of the curator of reptiles, Dr. Raymond L. Ditmars.

The main object of our study with these animals was to measure the heat production and the body temperature under various conditions, and to compare the results with similar observations on humans and other warm-blooded animals. Many other observations were likewise made regarding changes in body weight, the heart rate, the respiration rate, the water vaporized from the body, and the composition of the urine and feces.

Since it was known from previous experimental work that the heat production can be most readily determined not by direct measurement but by indirect measurement of the products of respiration, an apparatus was installed in the reptile house of the New York Zoological Park in which the carbon dioxide exhaled and the oxygen absorbed by the snake or other reptile could be accurately measured during a definite period of time.

In addition to the study of the heat production, another problem was to establish the relationship between the body temperature and the environmental temperature of these animals. Seemingly it should be simple to establish this relationship but, as a matter of fact, it proved to be most difficult. The temperature of the animal itself could



THE APPARATUS

USED BY THE NUTRITION LABORATORY AT THE NEW YORK ZOOLOGICAL PARK TO STUDY THE HEAT PRODUCTION OF SNAKES AND OTHER REPTILES. THE SNAKE IS PLACED IN AN AIR-TIGHT, WELL-VENTILATED BOX, SHOWN BACK OF THE TABLE, COVERED WITH A QUILT. THE BLOWER UNDER THE TABLE DRAWS THE AIR FROM THE CHAMBER, PASSES IT THROUGH THE REAGENT BOTTLES ON TOP OF THE TABLE, WHERE THE CARBON DIOXIDE AND WATER VAPOR ELIMINATED BY THE SNAKE ARE REMOVED FROM THE AIR, AND RETURNS IT TO THE CHAMBER, ENRICHED WITH OXYGEN TO REPLACE THAT USED BY THE SNAKE. THE INCREASES IN WEIGHT OF THE VARIOUS REAGENT BOTTLES (WHICH ARE WEIGHED ON THE BALANCE IN THE REAR) SHOW THE AMOUNT OF CARBON DIOXIDE EXHALED AND WATER VAPOR ELIMINATED BY THE ANIMAL.

MEASURING THE SKIN TEMPERATURE OF A LARGE PYTHON

AT THE NEW YORK ZOOLOGICAL PARK WITH AN ELECTRIC THERMOMETER. WHEREAS THE BODY TEMPERATURE OF MAN AND PRACTICALLY ALL WARM-BLOODED ANIMALS REMAINS ESSENTIALLY UNCHANGED REGARDLESS OF OUTSIDE TEMPERATURE, THAT OF COLD-BLOODED ANIMALS IS AFFECTED BY THE TEMPERATURE OF THEIR SURROUNDINGS.

be accurately determined by noting the temperature in the mouth or the cloaca, but the exact temperature of the air surrounding the animal was not so easily found. For this reason mercurial thermometers were not used in our most exact studies, but, instead, a sensitive electric device, whereby the temperature of the skin of the snake and the temperature of the air around the snake could be recorded within a few seconds.

In the accompanying picture we see a man at the door of the snake den in the New York Zoological Park, holding in his hand a light bamboo pole, at the end of which is a so-called "thermo-junction" or electric thermometer. This electric thermometer is connected by wires to another electric device (galvanometer) in the room outside the den. The electric thermometer was carefully placed between the folds of the snake, and the temperature of the skin at that spot was instantly recorded on the apparatus outside.

Fifteen seconds after this particular photograph was taken, this huge snake (a python) made a lunge at an observer at the front of the den, striking its head against the plate glass at the front of the cage but fortunately missing the man. Many of these snakes are highly irritable and dangerous to handle.

The body temperature of man and practically all warm-blooded animals remains essentially constant under normal conditions, regardless of what the environmental temperature may be. In the cold weather of the Arctic zone, for example, the body temperature of the warm-blooded animal is no lower than it is in the temperate zone or the tropics. Indeed, the polar fox is known to have a body temperature even higher than that of man. The temperature of the cold-blooded animal, on the other hand, is affected by the temperature of its surroundings.

Thus, the Nutrition Laboratory found that the body or the cell temperature of the snake changes with almost mathematical regularity with the changes in room temperature. It has been supposed that the cold-blooded animal has a temperature a little *above* its environment, but our observations prove conclusively that the temperature of the snake is usually a little below that of the environment.

It was likewise found that the snake loses a great deal of water from its skin and lungs. This water, in vaporizing, absorbs heat. The total amount of heat

produced by the snake is usually so small that all of it is lost in the vaporization of water. In many instances heat may be absorbed by the animal from the environment, as it suns itself. This absorbed heat serves to vaporize still more water.

Under ordinary circumstances the quiet snake, not in active digestion, will have a temperature only a degree below the average temperature of the environment. At the peak of digestion there may be a slight elevation of its temperature. If the snake is agitated or has been extremely active, its temperature may be 9° or 10° F. above the temperature of the environment but within a few hours will fall to the temperature of the environment or somewhat below it.

One of the most interesting features of our temperature study was with an incubating snake. The majority of snakes are viviparous, some are oviparous, and of these one is known to cover her eggs with her body. It has long been a matter of debate as to whether this covering of the eggs by the mother snake is primarily to protect the eggs or to aid in the incubation.

In April, 1931, a South African python laid 20 eggs at the National Zoological Park in Washington, D. C., coiled about them in the characteristic manner, and remained there for approximately 60 days. Unfortunately the eggs were not fertile and hence did not hatch. The Nutrition Laboratory was privileged to study the skin temperature of this animal while she was on her eggs.

Extreme care was taken to establish the true temperature of the air surrounding the snake, indeed even greater

AN AFRICAN PYTHON GUARDING HER EGGS

A FEMALE AFRICAN PYTHON AT THE NATIONAL ZOOLOGICAL PARK IN WASHINGTON, D. C., COILED AROUND A CLUTCH OF TWENTY EGGS. SHE REMAINED IN THIS POSITION FOR SIXTY DAYS BUT THE EGGS WERE NOT FERTILE AND DID NOT HATCH. SCIENTISTS HAVE BEEN IN DOUBT WHETHER THE MOTHER COVERS THE EGGS FOR PROTECTION OR FOR INCUBATION. CAREFUL MEASUREMENTS SHOWED THE TEMPERATURE OF THE AIR SURROUNDING THE SNAKE TO BE 86° F. WHILE THAT OF ITS BODY WAS 93° F. APPARENTLY THE MOTHER-SNAKE WAS PRODUCING HEAT TO KEEP HER EGGS WARM.

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In April, 1931, a South African python laid 20 eggs at the National Zoological Park in Washington, D. C., coiled about them in the characteristic manner, and remained there for approximately 60 days. Unfortunately the eggs were not fertile and hence did not hatch. The Nutrition Laboratory was privileged to study the skin temperature of this animal while she was on her eggs.

Extreme care was taken to establish the true temperature of the air surrounding the snake, indeed even greater

AN AFRICAN PYTHON GUARDING HER EGGS

A female African python at the National Zoological Park in Washington, D. C., coiled around a clutch of twenty eggs. She remained in this position for sixty days but the eggs were not fertile and did not hatch. Scientists have been in doubt whether the mother covers the eggs for protection or for incubation. Careful measurements showed the temperature of the air surrounding the snake to be 86° F. while that of its body was 93° F. Apparently the mother-snake was producing heat to keep her eggs warm.

care than was taken to note the temperature of the snake itself. It was clearly demonstrated that the body temperature of this snake was on the whole about 7° F. above that of the air surrounding her. The snake was placid and tractable during the observations, and hence the measurements were uncontaminated by any irritability, excitation or muscular activity, which of themselves would increase her temperature. It thus appears clear that this snake's temperature actually was above that of the environment, and seemingly she was producing heat to keep her eggs warm.

The cold-blooded animal's body temperature and heat production increase and decrease with the rise and fall of the temperature of the air around it. For the most part the cold-blooded animal is quiet in comparison with the warm-blooded animal. At low temperatures especially it engages in little muscular activity, and its vital activity is at a very low level. During periods of dormancy or hibernation its heat production reaches an almost immeasurably low level. It therefore needs very little food, compared with the warm-blooded animal. Yet the large serpents often eat an enormous meal at one time. Thus, during our study, a 70-pound python ate a 20-pound pig. In other words, at one meal it ate 400 times its daily energy needs or enough to last it a year.

In the wild the snake travels a relatively short distance to some water hole, where it waits until its prey comes along. The attack is immediate, and for perhaps 20 minutes the activity is vigorous. At the end of this time the prey has usually not only been killed but is completely swallowed. The snake now has enough food to meet its needs for possibly several months, and has used up very little energy in obtaining it. With no other animals in the world, certainly among the vertebrates, can one expect such economy in the securing of food. In view of this economy in energy expenditure, one can easily understand why it is possible for a large snake to exist for a year or longer without food.

With warm-blooded animals it has been found that, in general, the larger the animal the larger the total heat production. This relationship, however, is not directly proportional to the size of the animal. Thus, a dog weighing twice as much as another dog will not produce twice the amount of heat; nor will a bird three times as large as another produce three times as much heat.

It was thought many years ago that the production of heat is determined by the possibility for losing heat, that is, by the surface area of the animal. Therefore, it was believed that the heat production is proportional to the surface area and that all animals produce the same amount of heat per unit of surface area. With the warm-blooded animals studied by the Nutrition Laboratory it was quickly found that this is not true, but that there is a considerable difference in the heat production per unit of surface area.

Among the cold-blooded animals, as is to be expected, the larger animals also produce the greater amount of heat. But here again the total heat production is not directly proportional to the body weight, for the smaller animals (as with the smaller warm-blooded animals) produce a larger amount of heat per unit of weight than do the larger animals.

On the other hand, the heat production per unit of surface area was for all the cold-blooded animals that we studied, other than the tortoise, reasonably constant at a constant temperature. In such comparisons with cold-blooded animals the temperature has to be taken into consideration as well as differences in surface area, because the heat production varies with the changes in temperature.

The above comparisons are perhaps of

MEASURING THE SKIN TEMPERATURE OF THE INCUBATING PYTHON SHOWN IN THE PRECEDING PICTURE. THE SNAKE WAS PLACID AND TRACTABLE DURING THE OBSERVATIONS, HENCE THE MEASUREMENTS WERE NOT AFFECTED BY IRRITABILITY, EXCITATION, OR MUSCULAR ACTIVITY WHICH OF THEMSELVES WOULD HAVE INCREASED HER TEMPERATURE.

more special interest to the physiologist than to the general reader. We are more particularly concerned in learning how observations on the snake, the lizard, the alligator or the tortoise can help to solve the problems that are found in human physiology. For this purpose we must compare the cold-blooded with the warm-blooded animal, preferably choosing two animals of the same weight. How does the heat production of two such animals of the same weight compare?

In order to make such a comparison both animals must have the same body temperature. It is practically impossible to lower very much the temperature of the warm-blooded animal under normal conditions. Only those animals that hibernate have a variable temperature, and such animals are difficult to secure for study. On the other hand, it is possible to warm the body of certain cold-blooded animals, especially the tropical animals, so that their cell temperature will be essentially that of the warm-blooded animal, that is, about 98.6° F. This was done with a number of snakes, both small and large, and it was found that the cold-blooded animal,

even at the same temperature as the warm-blooded animal and weighing the same, can not produce the same amount of heat that the warm-blooded organism produces.

The Nutrition Laboratory has found that the mouse, for example, one of the smallest mammals, produces 640 calories for each square meter of its surface area under normal conditions, without muscular activity, and at an environmental temperature of 82° F. The snake, on the contrary, even when it has the same body temperature as the mouse, can produce at the most only about 100 calories for each square meter of surface area. There is thus a tremendous difference in the thermal relationships shown by these two types of animal.

In this gap between cold-blooded and warm-blooded animals three striking intermediary steps have been noted, further study of which will probably throw much light upon the pronounced differences between cold-blooded and warm-blooded animals.

The female python, for instance, has a body temperature actually higher than its environment during the incubation period. It will be interesting to determine what its heat production is during this period and whether it approximates that of warm-blooded animals any more closely than does that of the non-incubating snake.

The tortoises likewise furnish a field for further study. They have a much higher heat production for each square meter of surface area than do the snakes, alligators and lizards. They also have a much richer blood supply than these other cold-blooded creatures, but even then the blood supply of the tortoise is too feeble to enable it to produce heat at the same rate as the warm-blooded animal. Nevertheless, it can produce more heat than the snake, lizard and alligator, and hence may be considered to occupy an intermediary stage between these animals and the warm-blooded.

The hibernating animal likewise represents an intermediary stage. During hibernation the warm-blooded animal's body temperature and vital activity are greatly lowered. Indeed, the body temperature of a hibernating warm-blooded animal may easily be the same as that of the snake, alligator or fish, for its temperature will fall with that of the environment nearly down to the freezing point of water without resulting in the death of the animal. It is therefore possible to compare the heat production of a hibernating marmot, for example, with that of a lizard of the same body weight and the same body or cell temperature.

A study with hibernating woodchucks is now in progress at the Nutrition Laboratory, but is not yet completed. Thus far the results are in full harmony with several studies of this type by other investigators, who have shown that the marmot even at the same cell temperature as the snake has a much higher heat production. Here again the animal having the richer blood supply and better blood distribution has the larger heat production and is living at a higher level of vital activity, even though its body temperature is greatly depressed. Such variations in body temperature as are found with the hibernating marmot and with the snake, alligator and lizard in the normal course of their existence are not found with humans.

A certain amount of fluctuation in the body temperature of humans does take place, however, especially under the abnormal conditions resulting in fever. Therefore, it is of direct value, in interpreting the laws governing the heat production and heat loss of humans, to study animals showing such marked changes in heat production and body temperature as the incubating python, the red-fleshed tortoise and the hibernating marmot. When such studies are completed, we may hope to be able to solve some of the still unanswered problems in human physiology.

RELATION OF GEOLOGY TO OCEANOGRAPHY[1]

By Professor W. H. TWENHOFEL

DEPARTMENT OF GEOLOGY, UNIVERSITY OF WISCONSIN, AND

CHAIRMAN, DIVISION OF GEOLOGY AND GEOGRAPHY, NATIONAL RESEARCH COUNCIL, 1931–32

Introduction

It is not known what proportion of the sedimentary rocks were deposited under oceanic conditions, but it is believed that a fair estimate of those exposed on the continents would assign at least 60 per cent. to a marine origin. An additional percentage of at least 20 was deposited on land in such close association to marine conditions that the deposition was affected by the marine waters. Such are the deposits made in and on deltas built into the sea and the accumulations of estuaries, lagoons and marine swamps and marshes. The geologic column, as applied to North America shows that most of the Cambrian, Ordovician, Silurian and Devonian systems, more than half of the Mississippian, much of the Pennsylvanian and Permian, considerable parts of the Triassic and Jurassic, much of the Cretaceous, and large parts of the Tertiary were deposited in the sea or in closely adjacent environments.

All the great limestones seem to be of marine origin. Such is also the case for most of the evaporites, as rock salt, anhydrite, gypsum, etc., although the conditions of deposition were not typically marine. In the sea seem also to have been deposited the bedded phosphorites of which those of the Rocky Mountain region are representative. Most of the flints and cherts certainly developed under the conditions of oceanic environments. The great iron formations of the Precambrian are generally interpreted as having been deposited under marine conditions. The oil fields of the world seem related in their origin to marine environments and such wide-spread black and oil shale formations as the Chattanooga, Utica, and Monterey, the generally regarded mother rocks of petroleum, were either deposited in the sea or in environments subject to marine influences.

The sea is the great settling basin for everything washed or blown from the land. It is the final goal of all sediments. It is the place where lived the majority of the organisms known from the geologic column without whose preservation stratigraphy would be a tantalizing and generally barren subject. The accumulation of these vast bodies of sediments in the sea sets in motion physical and chemical processes in the accumulating sediments and the materials upon which they rest. Sequels to such deposition are thought by many geologists to be elevation of parts of the continental areas.

Thus, with so many of the geologic products forming in, or in close connection with the ocean, it is obvious that intimate relationships obtain between oceanography and geology. These relationships will be briefly examined from several points of view with the greater emphasis placed upon those fields of contact with which the writer has familiarity. Different products will be considered from the point of view of single causal factors. This is done for the sake of simplicity, but it should be understood that most, if not all sedimentary materials require the concurrence of several factors in their formation.

[1] Fourth paper in a symposium, "Major Problems of Modern Oceanographic Research," at the meeting of the American Association for the Advancement of Science, at Pasadena, California, June 17, 1931.

Relation of Geology to Marine Biology

Many marine sedimentary rocks in large or small part are composed of organic matter consisting of the shells, bones, and tests of organisms. These organic materials were formed by animals and plants living in the sea where they were controlled during life and during their evolution by oceanic conditions. The organisms were either invertebrates or vertebrates, the latter interesting and having some geologic importance, the former of extremely great geologic significance. Among other points of view, marine organisms must be studied for determination of the products they are making and the conditions under which the products are made in order that the results of the study may assist in interpreting the rocks and the organisms of the geologic column. Our knowledge of coral reef lithology and faunal distribution over coral reefs and in the adjacent waters was largely derived from the oceanographic work of Darwin, Dana, Murray, Saville-Kent, Vaughan and others, and without this oceanographic work much of the coral reef geology of past geologic periods could not have been interpreted. The varieties of deposits about the reefs —the muds of the lagoons, the sands and gravels in the channels, the coral sands and muds seaward from the reefs, the structureless bodies of the reefs themselves, and the distribution of corals and other organisms on the different parts of a reef—had first to be understood in and about living reefs before the ancient reefs could be interpreted. The work of Howe and Setchell on the contributions of the algae to reef rock and of Walcott on the Precambrian algae has been only partially understood, and more emphasis on this phase of rock-making should be made until it becomes fully appreciated what the algae are doing in modern seas so that their work in ancient seas may be evaluated. The interpretation and appreciation of the algal reefs have lagged in America because of the ignorance or inappreciation of geologists of the great importance of algae in rock building. Since the publication of Pia's great work there is no longer any excuse for such neglect.

The invertebrate life of the sea belongs to the benthos, plankton and nekton. The nekton are not particularly common in the geologic column and do not have a great deal of practical geologic value. The benthos and plankton are of great importance to geology and through them has been constructed in large part the time table of the geologic history of our globe. Each period of earth history is known to have been characterized by a definite assemblage of organisms, and each smaller division was likewise so marked. Most of the fossils in geologic laboratories and those studied by students of geology are invertebrates of marine origin. Stratigraphy would have myriads of problems impossible of solution were it not for the assistance of fossil organisms.

Each of these organisms lived under particular conditions, surviving at a place so long as the conditions permitted and no doubt migrating elsewhere as conditions to which the species was adapted were extended or were moved from one place to another. It needs to be known what are the conditions under which modern marine organisms dwell in order that the environmental conditions under which the ancient organisms lived may be evaluated. It has been assumed that organisms during each division of earth history had quite general distribution in the oceanic waters. Some slight allowances were made for isolation, or provincial development. Oceanographic research has shown that the primary assumption can no longer be made, and reflection should be con-

vincing that it never should have been held. It is known that marine organisms in their distribution are adapted to and controlled by various factors of the environment. Some benthonic organisms are controlled by the character of the bottom, others by depth of water, and others by temperature, light, muddiness, agitation, food supply, low or high salinity, presence of enemies, and other factors. The problems are extremely complex and it seems doubtful if they are completely understood for a single marine environment or for many organisms. The most important factor for the benthos seems to be the character of the substratum or the bottom. Thus, bottoms composed of gray muds have characteristic organisms, black muds have a somewhat different assemblage. Bottoms that are foul are not lacking in organic matter, but this has been developed in overlying or adjacent waters and not at the place. Sand bottoms have a distinct assemblage; similarly bottoms covered with gravels, solid rock bottoms, and those bottoms overgrown with marine vegetation. Some bottoms are colonized by millions of organisms, others, as some sand bottoms and bottoms of black muds, may be veritable marine deserts. Likewise, bottoms subject to changing temperatures may be barren of life, and such is also likely to be the case for those bottoms over which the salinity of the water is subject to wide variations.

Examples of differences in organic association according to environment may be cited as follows. The areas in some estuaries where the twice daily tidal change of water from fresh to salt and the reverse makes an impossible condition for most organisms. The work of Kindle on the organisms in the Bay of Fundy, Walther on Tauben Bank in the Bay of Naples, and Vaughan on the corals of the Cocos Keeling Islands are illuminative on this point, but this work may be said to have just begun.

The abundance of organisms over a bottom is not the only determining factor relating to their occurrence in the geologic column. The problem of the work of scavengers must be given consideration. There are marine bottoms where everything may be expected to be eaten many times. Thus on the coast of Northumberland, England, the bottom to a depth of 24 inches is said by Davidson to make a complete passage through the intestinal organs of lobworms once each 14 months. It follows that before burial to a depth below which the lobworms work, the sediments will make many passages through the intestinal canals. Certain organisms eat everything and Verrill has described bottoms off the coast of New England abundantly populated with benthos, and overlying waters with nekton and plankton, but the dead organic materials covering the bottom are thoroughly chewed to pieces by the scavengers. There are many limestone formations in the geologic column which are shown by their sedimentary structures to have been deposited in waters not subject to agitation of great moment. Many of these show few fossils to the eye, but thin sections prove them to be abundantly filled with fragments. This breaking must be assigned to scavengers. Bottoms with abundant life are certain to have a part of that life devoted to eating the remains of the other parts so that a bottom may be densely populated and yet leave essentially nothing entire for burial in the sediments. Other bottoms with scanty life, but few scavengers, may have abundant remains subject to burial. Some bottoms may support organisms with difficulty because of the frequent influx of muddy sediments, by which at times the bottom assemblages are buried and smothered. Such events render bottoms extremely unfavorable for benthonic organisms, but favor their burial and hence preservation. A muddy condition neither too favorable, nor too un-

favorable for benthonic life seems to give greater chances for preservation of organic remains, as the influx of mud may kill the scavengers at the same time as the organisms upon whose dead remains they feed.

Plankton in their adaptation are not so much directly affected by the character of the bottom and the same is true of the nekton, but the latter and, in some cases, also the former are dependent upon some of the benthos for food so that they are indirectly affected.

Thus marine organisms live in colonies adapted to the environment and their abundance on the bottom during life is directly related to those conditions favoring their presence, but their abundance in the rocks of the geologic column is determined by those conditions which permit their preservation after death.

Again the presence of organic remains in sedimentary rocks may indicate little with respect to habits or places of life. In life certain environmental factors determine whether benthonic organisms are present or not; but after death, all organic remains are as inorganic sediments, and movement is dependent upon the competency and capacity of the oceanic waters, and these waters may carry the organic remains to places where the living organisms could not have dwelt. Many examples of shells of existing marine organisms being transported to places impossible for the dwelling of the living animals are known. Thus, on Dog Bay on the coast of Galway, Ireland, the shells of protozoa are carried several miles inland from the sea; the same thing happens to a less degree on the coast of Martinique; on the island of Anticosti crows and gulls carry the testa of the common sea urchin several miles inland and they also carry the shells of the common black mussel, *Mytilus*, equal distances landward from the coast. Currents may bring abundant plankton into the fresh or nearly fresh waters of estuaries and into the lagoons of paralic swamps. The animals perish there, their remains sink to the bottoms and, under conditions of foul or uncongenial bottoms, scavengers are wanting and remains may be preserved in great abundance. In the sound between the Island of Moon and the mainland of Esthonia the bottoms are foul with black mud carrying hydrogen sulphide and living organisms are few. The occasional storms bring in myriads of plankton and the planktonic young and small shells of benthos so that these small shells must be filling many layers. Some day these may be found by some yet to be born geologist and if he base his conclusions merely on the presence of the shells he may assume the bottoms to have teemed with dwarf forms. It is thought that the black shales with abundant planktonic graptolites do not represent the natural environment in which the organisms lived but the abnormal one in which they died.

Thus the marine biologist must give to the geologist not only information relating to the life, products, habits and habitats of the marine organisms but also what happens to their remains after death. He must not only give information relating to the scavengers of the bottom, the places and conditions where organisms are most likely to be buried without removal from the place of life and without destruction of the shell matter, but also to what places they may be transported and what are the evidences of the places of finding being those of the living environment or the places to which the remains were transported. Geologists await this information with expectancy.

When marine biologists have given to geologists the many facts relating to organic distribution, both living and dead matter, many of the data of stratigraphy will no doubt receive different interpre-

tation and many divisions of rocks now interpreted as sequential will be interpreted as arising from lateral variations of the environment and hence contemporaneous. Time relations of some fossil organisms may then be given a somewhat different setting.

Relation of Sediments to Depths of the Ocean

The variety of sediments attaining deposition on oceanic bottoms is conditioned by depth as one factor, this applying to organic as well as inorganic sediments. A few have advocated that deposition in most of the Paleozoic epicontinental seas was uniform from the point of view of the character of the sediments. This view seems so absurd as to be unnecessary of comment. It has also been the view that deposition over an ocean bottom was not subject to serious interruption except by fall of sea-level or building to the surface. Minor interruptions occasioned by limits of supply are not considered. The interruptions caused by fall of sea-level are not debatable providing the fall of the sea brings the bottom above the sea surface, but some doubt exists as to whether it is possible to build the bottom to the surface at many places. Two groups of agents from the point of view of the deposition of the sediments are operative at every place on the ocean bottom and whether or not sediments are deposited depends upon the relations of these two agents to each other. The two groups of agents are complex and on the one hand are concerned with those factors upon which the supply of sediments depends and on the other upon those which are concerned with the disposal of the sediments. Sediments are brought to a given place. If the agents concerned with disposal at that place are equal to the task of removing these sediments no deposition occurs and the sediments are transported to places where the agents of disposal are not equal to those of supply. Such places are in the deeper waters. If the agents of supply exceed the ability of the agents of disposal, the excess is left at the places concerned, a deposit results, and the bottom through this deposit is raised. This brings the bottom nearer to the surface of the water and normally this increases the disposing ability of the waves and currents and deposition at that place is correspondingly decreased. Ultimately the disposing ability of the given place will equal supply and the two groups of agents are in equilibrium. Nothing then attains permanent deposition, and the bottom remains in a constant position with respect to sea-level, the disposing agents can do no more than carry away that which is brought. The position of the bottom is fixed for the conditions and it has attained the position of the profile of equilibrium. When that position has been attained each particle of sediment brought from the sources of supply decreases normally those which remain to be brought with the result that that supply is progressively decreased, but as there is not likely to be decrease in disposing ability the consequence is that the excess in the agents of disposition may be utilized to remove some of the sediments previously deposited and the bottom will ultimately be lowered to some limit determined by the depth at which the disposing agents do not affect the bottom. This is the marine base level of erosion and also the base level of deposition. It is obvious that the level of each profile of equilibrium is determined by the balance between the agents of supply and those of disposal, or putting it in economic terms, by the balance between supply and demand. With large supply relative to disposition factors, the position of the profile of equilibrium becomes high; with small supply relative to the same factors the position of the profile is low. The

depth of the base level of erosion is determined by the strength of the disposing agents, the waves and currents, and in the open ocean its depth for a given distance from the shore is probably bounded by somewhat narrow limits.

In epicontinental seas a somewhat different condition exists. The agents of disposal have a limit to their activities, in that the deeps below the base-level of erosion may not exist, or ultimately become filled, so that deposition must take place and thus there is no base-level of deposition and no profile of equilibrium nearer than some level above the water surface and there is little chance of a sinking profile of equilibrium after the central basins have become filled.

Each position of the profile of equilibrium and also the position of the base level of erosion mark breaks in the geologic record and there must be many of these, the duration being either of the nature of diastems or unconformities. In the general run of events most are probably of the order of magnitude of diastems. Deep profiles of equilibrium and the position of the base-level of erosion are most likely to be attained during those times of continental stability when the continents are deeply eroded or even reduced to base-level so that such times should have great stratigraphic breaks developed beneath the level of the sea. Great unconformities on the land shown by overlap of the sea should have continuation developed beneath the surface of the sea.

In this field the geologist needs much assistance. Little attention has been given to breaks in the record which mark times of no deposition or marine erosion. The general tendency has been to assign every break to a land interval, but if the reasoning outlined has validity there should be fully as many breaks that developed beneath the sea. Much work needs to be done to determine the relations of deposition to the bottom. There needs to be determined how extensive are the bottoms receiving deposits, what parts are in equilibrium and stationary, and what parts are being eroded. A little work has been done in this field, but not enough to permit determination of the extents of the stratigraphic breaks that are being developed. The most important piece of work of this character done in America seems to be that done in Chesapeake Bay about the mouth of the Choptank River. Since the location is near the mouth of a river and the areas of erosion and non-deposition are large, 8 per cent. of the bottom being eroded, 66 per cent. being stationary, and only 26 per cent. receiving deposits, it may be concluded that over shallow bottoms of the sea where no large additions of terrigenous sediments are being made there should be extremely extensive areas of non-deposition or erosion.

The variety of sediments that attains deposition is also related to depth. Thus over the deep ocean the bottoms are not congenial to a great variety or number of organisms and between the surface and the bottoms there is vast depth of water through which materials derived from the surface must sink before reaching the bottom. The bottom waters of the deep oceans have come in considerable parts from the polar regions and are thus cold, in which condition they can hold large quantities of gas in solution so that these bottom waters are likely to be adequately provided with oxygen and carbon dioxide. The results are that the bottom waters are oxidizing and at the same time have high solvent action for carbonates. Material coming from the surface is in small particles, probably settles slowly, and has large opportunity to be dissolved and completely oxidized. The results are that carbonates are likely to be taken into solution, even silica is dissolved to a greater or less degree, and the materials deposited are largely hydrous aluminum

silicates, aluminum hydroxide, iron hydroxide, fragments of quartz, and other extremely insoluble substances. These materials with some carbonates compose the red clay of the deep abyss. Shallower depths of the deep ocean receive soluble materials from the surface because the height of the column of water through which they sink is not sufficient to take them into solution. Organic oozes may then be deposited over those bottoms where the quantities of other sediments are not so great as to mask the planktonic materials composing the oozes. Still shallower depths receive large quantities of muds brought from the continental areas, these having been brought because the currents at and above those depths were of competency adequate to bring muds and were not of such competency as to bring coarser particles. The presence of these muds masks the planktonic organic sediments and may prevent colonization by the benthos. Thus the organic sediments do not make a serious impress and the deposits are muds of various colors as green, red, gray and blue. These cover extensive areas over various parts of the ocean bottom from the littoral to beyond the edge of the continental shelf. Depth of course is not the only factor in their deposition, but it is an important factor.

Where the terrigenous sediments are not so great and the other conditions are congenial, organic sediments from benthonic animals and plants may dominate in the materials deposited. These are largely carbonates and give rise to limestones, generally interpreted as of deep water origin, but with little evidence suggesting that generalization. Most of the evidence is in harmony with the view that limestone formation by organisms is favored by shallowness of water, in that this is the zone of light, and the other agents that assist in the deposition of lime carbonate seem most likely to be found in shallow water. The coral and algal limestones are quite certainly of shallow water deposition, the former in depths not exceeding 30 to 40 fathoms and the latter limited by the depth to which light may penetrate to enable the algae containing chlorophyll to thrive and utilize the carbon dioxide of the water and in this way precipitate calcium carbonate. How deep this may be done should be carefully determined. Bottoms which are within the zone of light may have immense populations of plants and their presence gives an aspect to the sediments which is not possible if plants are absent. The bottoms of past seas in places became plastered with lime made by algae giving rise to such deposits as those composing the Kona dolomite of the Lake Superior Huronian, some of the limestones of the Precambrian of the northern Rockies of Montana, parts of the Oneota dolomite of the Upper Mississippi Valley, parts of the Arbuckle limestone of Oklahoma, the great limestones of the Triassic of Tirol, and some of the great limestones of the Permian Basin of West Texas.

Holes in the sea bottom of considerable depths below their surroundings have little circulation and are likely to become poor in oxygen. Fine inorganic sediments and organic matter are likely to move to such holes, thus filling them with black muds. The low oxygen content of the filling waters keeps out the scavengers and aerobic bacteria whose presence would eliminate the organic matter and the blackness. Anaerobic bacteria fill these sediments and the presence of some of these lead to reduction of sulphates and the formation of hydrogen sulphide and ultimately the sulphides of iron. The work done by Trask on the sediments about the Channel Islands of the California coast emphasizes the abundance of fine sediments in the holes, whereas the ridges

between the holes are characterized by coarser sediments which remain there because the transporting agents on the ridges do not have sufficient competency to carry these coarser sediments to the holes. Goldman has called attention to the black muds filling deep holes in Chesapeake Bay and it seems quite certain that most deep holes of circumscribed area on the shallower parts of the ocean bottom receive fine sediments which ultimately become colored by organic matter.

The great dolomite formations probably bear some relations to depth and also probably to temperature. To the extent that these dolomites are the result of the replacement of calcium carbonate —and most geologists accept the view that the great formational units not affected by magmatic action are of replacement origin—it may be considered established that the original deposition of the calcium carbonate was related to shallow depths. It is generally assumed that the replacement took place in shallow and warm waters and that such is not possible in deep and cold waters. It has been stated that under conditions of atmospheric pressure calcium carbonate is more soluble than magnesium carbonate, but that under pressures of five atmospheres the magnesium carbonate is the more soluble. While observations are not wholly in accord with these generalizations, they may be accepted as more or less approximately correct. The generalization limiting replacement to pressures of less than five atmospheres makes the places of its occurrence on bottoms of shallow depths. If dolomite is ever extensively precipitated as an original sediment it would seem that shallow depths must necessarily obtain, as the double carbonate would have to be deposited where the two carbonates approached equal solubility unless the deposition was affected by organic agencies.

Oceanic research is nearly inarticulate on the dolomite problem and geologists are keenly listening for some words of assistance. The voice of oceanography is also needed to give geologists more definite data on the relations of varieties of oceanic sediments to depth.

Relation of Sediments to Rates of Deposition in the Sea

The various rates of deposition for different depths are almost altogether unknown. It is assumed that a slow rate of deposition would lead to chemical (diagenetic) changes in the sediments due to the long exposure to the materials in solution and to the work of the organisms upon them. Assumptions have been made that limestone is deposited more slowly than shales or sands, but the assumptions are little more than guesses. The rates of deposition in the deep sea have been thought to be almost negligible. The writer has suggested reasons for believing that the rates may be much larger than generally supposed and that the minimum rate may be of the order of one foot in about 80,000 years, a very slow rate, but considering the immense period of geologic time and the great area of the ocean basins that rate is competent to account for the deposition of greater quantities of sediments in the deep sea than are known upon the lands. It has generally been assumed that the rates of deposition in shallow waters of such sediments as sands must be rapid because they contain ripple marks, cross-lamination, and other evidences of wave and current action. But there are few good reasons for assuming rapid deposition of sands as of general application. As pointed out by Evans, the rates of deposition of some sands may be far slower than the accumulation of any other variety of sediment. A bottom receiving a small supply of sands could not have a considerable thickness accumulated rapidly

and before a rapid rate of deposition should be assumed it should be shown that there were large quantities of sands supplied during short periods of time. A bottom at or near the profile of equilibrium and remaining so for a long time might have a few inches or a few feet of sand over the bottom and these might have been deposited and redeposited for thousands of years so that the few inches might represent thousands of feet of any other sediment deposited over bottoms in large quantities and below the profile of equilibrium. Sands deposited on a bottom near the profile of equilibrium with sea-level slowly rising could be deposited no more rapidly than sea-level rose and if this were slow the accumulations might be an inch in a thousand years or ten thousand years and yet these sands could have all the signs of rapid deposition so far as ripple marks and cross-lamination are concerned. Such slow deposition might also be the case for muds or any other type of sediment, organic or inorganic. It is hoped that oceanographic research ultimately will yield the approximate rates of deposition of different kinds of sediments in different depths of water, at different distances from the shore, under other varying conditions, and also give to the geologist criteria for determining whether deposited slowly or rapidly.

Glauconite is a variety of sediment which seems to have its origin conditioned by a slow rate of deposition so that the constituent materials deposited may exist for a sufficiently long time on the bottom to bring about the chemical adjustments resulting in the mineral. Its origin was originally referred to foraminifera, but there is little evidence that these organisms are causally concerned. It seems probable, however, that organisms play some indirect part in the origin as evidence is developing that the glauconite grains represent excremental pellets, and Buchanan and the two Japanese students, Takahashi and Yagi, have described pellets showing the transitions from particles definitely recognized as excremental to those completely glauconitized. In order to permit these adjustments it would seem that slow accumulation of sediments is essential. The bedded phosphorites contain many spherical particles and it has also been suggested that these pellets are of excremental origin which later became phosphatized because of the chemical nature of the sea water and unknown physical factors.

The formation of dolomite may result from conditions of slow deposition as such subjects the original calcium carbonate to leaching and the replacing influence of magnesium carbonate in solution. This is not known, but it may be a lead worth following. It may be that flint and chert are consequences of slow deposition. Under conditions of rapid deposition the silica carried by the streams to the sea would attain wide dissemination throughout other sediments and thus it would not be possible for the colloidal silica to collect in the form of nodules. Under conditions of slow deposition such collection would be possible and thus the nodules could result.

Slow deposition would also make it possible for sediments to become oxidized or reduced. On bottoms containing much plant matter other sediments would be likely to be reduced by the organic materials. Under conditions of rapid deposition the quantity of plant matter might not be sufficient to bring about this result. If the waters contained an adequate supply of oxygen and limited plant and other organic matter, sediments would experience oxidation.

To all the above problems oceanography may some day give the answer and thus render possible the solution of some of the geologists' most perplexing problems.

Relations of Sediments to Temperatures of Oceanic Waters

Oceanographic research has shown that the calcareous sediments are being deposited in greatest magnitude in the warmer waters of the sea, but it does not seem to have been shown what are the quantitative relations of deposition under different conditions of temperature. It is known that some calcareous sediments are being deposited in polar temperate waters, but the quantitative comparison of these with those deposited in warmer waters has not been determined.

It has been suggested that such great black shale formations as the Utica and Normanskill are the deposits of cold waters. Such does not seem likely, but it may be the case. Perhaps such bedded phosphorites as those of the Rocky Mountain region may represent cold conditions, but facts are lacking.

It is known that organisms are adapted to the various temperatures of the oceanic waters and much research has been done along these lines but the application of the data to the fossil organism has permitted various interpretations. The reef corals do not endure a temperature colder than 68 degrees F. at the present time. Do the reef corals of the past have the same temperature significance? This remains to be proven and it is expected that oceanography will give the answer.

The terrigenous sediments of high polar latitudes are largely of mechanical origin and thus have colors related to the colors of the fine particles of the rocks of derivation. Such fine particles are dominantly of light colors and thus it may be expected that the sediments deposited in polar waters would have similar colors. But the colors of many sediments of warm oceanic waters are also light. It may be expected that some day oceanography will produce the data permitting from the colors and other characters of the sediments the determination of the temperatures of the waters in which the sediments were deposited.

Temperature also aids in the precipitation of calcium carbonate as warm temperatures release carbon dioxide and thus may make possible the deposition of some lime. There are no quantitative data on this point.

Relation of Sediments to Water Agitation

It is through agitation of the waters of the ocean that sedimentary materials are transported, bottoms are stirred, and ripple marks and other sedimentary structures formed. These tell of depths, but it is not yet known to what depths agitation extends nor are the natures of the various sedimentary structures formed in the various depths sufficiently understood.

Information is desired with respect to the quantity and quality of suspended matter transported in ocean waters far from land as such data will assist in determining the rates of deposition over the deep sea.

Agitation of the waters also has relationships to the life of the bottom as thus oxygen is brought to lower levels and life otherwise excluded becomes possible. On the other hand, bottoms subject to frequent stirring yield much suspended matter to the overlying waters and thus bring about unfavorable conditions to some organisms, but favorable conditions to some others. Agitation also aids in expelling carbon dioxide and this leads to precipitation of calcium carbonate in waters saturated therewith. The extent to which calcium carbonate is precipitated in this way does not seem to be known.

Relation of Sediments to the Chemistry of the Sea

This is a large field and one in which not a great deal seems to be known. Sea

water seems to be an extremely complex material and the presence and abundance of the various things in solution are related to each other, the temperature, the life in the waters, the presence of matter in suspension, and many other factors. But the precipitation of the things in solution determines some of the things which may be deposited. Under a given pressure calcium carbonate is said to be more soluble than magnesium carbonate. Obviously, if this is true, whether one or the other is deposited or one replaced by the other will depend upon the pressure if that is the only factor considered. When this chemistry is worked out the dolomite problem will be solved. Why were phosphates deposited in large quantities over parts of the ancient seas? Again this is probably a problem of the chemistry of the ocean. The deposition of the fine-grained calcium carbonate of the waters about the islands of the West Indies has concerned geologists for many years and this too is connected with the chemistry of the waters. It is known that sea water has little silica in solution or in the form of colloids. The rivers carry much to the sea but what becomes of it is not known except that it must be precipitated shortly after reaching the sea. It is not known how it is precipitated and why it is that some limestones in a given sequence are full of chert nodules and others have few or none. Obviously, in some way this is connected with the transportation of silica into the ocean. The writer has suggested that the limestones full of chert nodules were deposited near the mouths of rivers flowing from a base-leveled land or a land unable to supply clastic sediments. Oceanographic research will some day yield the deciding facts.

Oolites are forming in the warm and shallow waters of many parts of the ocean. They also form in some salt lakes, as Great Salt Lake of Utah. Many hypotheses have been advanced for their presence, some referring them to evaporation, others to chemical precipitation around nuclei, others appeal to colloids for the formation, and their origin has also been assigned to the action of plants. There are not a great many facts, but there is little doubt that oceanographic research will some day solve the problem and give to geologists the significance of this variety of sediment.

Other chemical products of the sea are glauconite, certain of the zeolites, as analcite and phillipsite, several manganese compounds, and various substances of such minor quantitative importance as the authigenic feldspars. There are many questions relating to all of these which remain unanswered and with respect to some of them essentially nothing is known as to their origin. This is the case for the feldspar crystals which are known to form under some oceanic conditions. There is so much in this field of oceanic chemistry—both the chemistry of the waters and the chemical changes in the deposits (diagenesis)—that is unknown that geologists must be patient if the replies are not forthcoming for a long while.

Relation of Sediments and Sedimentary Structures to the Topography of the Ocean Bottom

There has been a tendency to assume that the bottoms of the ancient seas were essentially flat, but many parts of the present sea bottom do not exhibit such flatness. It may be assumed that the present sea bottom surfaces are abnormal in this respect, but it would seem more reasonable to assume that interpretations of the character of past sea bottoms are not correct.

The inclinations and irregularities of the surfaces of deposition are important for geology in that the thickness of sediments which may accumulate on a sur-

face is determined by the steepness of inclinations. Slopes that are too steep will not permit the accumulations of sediments, these sliding or gliding to lower levels and frequently becoming deformed in the movement. Data are accumulating to indicate that such gliding with attendant deformation may have been of not infrequent occurrence on the bottoms of ancient seas. Mapping of the irregularities of the shallow water areas of the existing seas coupled with experimental work in the laboratory will show, on the one hand, the inclination of the bottom and the extents of such inclination and, on the other hand, the angles at which various sediments will not remain on a surface. Oceanographers will produce the one line of information; geologists must find the other.

As already noted depressions in the ocean floor become sites of deposition of fine muds many of which are black from organic matter. High places tend to have coarser sediments. Faunas are adapted to each type of sediments as well as to the factors of light, depth, etc. The applications to stratigraphy and correlation are obvious.

Relation of Oceanography to Other Fields of Geology

With respect to other fields of geology the writer can advance his views with less confidence. However, it seems probable that petroleum and natural gas have some connections with oceanographic conditions. The sediments in which the original materials of the oil and gas were deposited seem likely to have been largely of oceanic deposition and oceanic conditions may have been responsible for the organic substances also. Trask has been working on this problem for the past several years and has discovered facts of importance. The great iron formations of the Precambrian are thought to have had their original materials deposited under oceanic conditions, the iron and silica being derived from magmas and precipitated on contact with the sea water. These are applications to economic geology. Similar applications exist for an appreciation of the building materials won from marine deposits, and the utilization of salt, potash and gypsum deposits, and other materials of oceanic origin.

Much has been written relative to a connection between great uplifts and the ocean. Thus one finds such statements as the pressure came from the Atlantic, the settling of the ocean bottom was responsible for the uplift. To what extent any ocean was responsible for any uplift remains to be determined. The movement of sediments from the continents to the sea fills to some extent the ocean basin and raises sea-level, thus bringing about appearance of settling of the continental areas.

The positions of the great basalt flows of the Deccan, Abyssinia, Paraná River Valley, the Columbia River have been interpreted as connected with the sinking of parts of the adjacent ocean bottom with attendant flow of the lava displaced by the settling to the edges of the settling blocks. The outflows of the Deccan and Abyssinia lavas have been correlated with the breaking down of the hypothetical Gondwana land.

Areas of depression in the sea, whether these seas are those upon the continental margins or those within the continents, are places of large accumulation of sediments and that these places of large accumulation later become loci of great deformation and uplift seems established. This was pointed out many years ago by Hall and has become almost axiomatic. Whether the uplift finds a cause in the accumulation, or whether both the uplift and the accumulation are effects of some unknown cause remains to be determined.

The origin of the ocean is a geologic problem not yet answered. There have been various hypotheses; some valid, others fantastic. Perhaps oceanography may some day find the answer.

Summary

There seems hardly any phase of marine sedimentary geology which in some way does not have connections with oceanic conditions and whose solution or extension is not related to the acquirement of information from the ocean. The greater number of fossil organisms known to geologists lived in the ocean. These fossil organisms constitute the chief means of correlation and they are the fundamental materials of paleontology. The greater part of the sedimentary rocks were formed in the sea, and each variety of rock is a response to some combination of oceanic conditions. The ocean in various ways has connections with economic geology and many important mineral deposits were formed in the sea and in some undetermined ways it has relations with the great lines of earth deformation and uplift. The problems are many; the solutions have been few.

SOME PROBLEMS OF OCEANOGRAPHIC CHEMISTRY[1]

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AND

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SCRIPPS INSTITUTION OF OCEANOGRAPHY

ONE of the factors of paramount importance in the study of the chemistry of sea water, is the selection of suitable analytical procedures. Unfortunately, the literature of the subject contains many articles based on unreliable methods of analysis, the use of reputedly good methods the experimental errors of which are too large to consistently note the minute variations taking place within the sea, and, worst of all, papers are occasionally encountered in which the chemical technique and knowledge displayed would even shame a sophomore. Many of the average analytical procedures do not apply to the study of sea water because of the interference produced by other substances present in the water and thus, in many cases, the methods must be subjected to further analytical refinement. This is particularly true of the analysis of the predominating ions occurring in sea water, as any change due to biological, physical, or chemical factors must necessarily be very small in relation to the large concentrations normally occurring in sea water. Such analyses call for refinements and techniques not usually demanded of the average analytical procedures.

To a certain degree similar statements may also be made for the determinations of the constituents existing in sea water only in very small concentrations; but with these the variations are fortunately relatively large, and the experimental errors of the methods are thus considerably nullified within certain limits.

It has been the practice in the laboratories of the University of Washington to make an exhaustive study of a method before applying it to the analysis of sea water. This is most reliably accomplished by the use of synthetic sea water prepared from carefully purified chemicals. Such a synthetic sea water contains all the major constituents of sea water with the exception of the one being subjected to investigation. Standard solutions of the particular constituent, prepared with all necessary precautions, are then added to the synthetic water in quantities normally existing for a given chlorinity. If a gravimetric process is employed, then spectroscopic examinations are made of the precipitate.

For the major constituents of sea water there are no methods of suitable refinement for field study, except that of the total halides, and such studies must accordingly be made in a well-equipped laboratory. However, for the minor constituents many methods have been developed, within a reasonable degree of accuracy, that can be readily applied in the field and especially aboard ship. Gravimetric methods are generally preferable in most cases because opportunity is offered to finally test the purity of the precipitates weighed. However, volumetric processes

[1] Second paper in a symposium, "Major Problems of Modern Oceanographic Research," at the meeting of the American Association for the Advancement of Science, at Pasadena, California, June 17, 1931.

are far more convenient and more rapid but interfering ions almost invariably cause difficulty. Thus the Mohr method for halides may be performed with a marked accuracy, as far as the total halides are concerned, but no distinction is possible between the chlorides, bromides or iodides. In this determination the halides are calculated as chlorides and under such circumstances no comparison of this method with gravimetric process is possible unless suitable corrections are made. In like manner the volumetric method for calcium will not agree, theoretically at least, with the gravimetric, unless certain corrections are taken into consideration.

Colorimetric methods for the determination of substances occurring only in low concentrations are almost exclusively used. The limits of accuracy and reliability of these methods are too well known for further comment. Too much emphasis, however, can not be placed on the proper control of conditions, especially when the results are to be compared with those of other investigators. Changes in temperature, variations in the amounts of dissolved substances and impure chemically-pure chemicals may often lead to erroneous conclusions, if not properly considered. In many of the colorimetric methods applied to sea water variations in the dissolved salts have a pronounced effect. Wattenberg has nullified such errors in his modified method for ammonia, Thompson and Bonnar used a similar procedure in the development of the determination of the buffer capacity of sea water and at the present time Dr. Carter, of the Pacific Biological Station, is perfecting the procedure for the estimation of nitrates.

The constancy of composition of sea water was postulated many years ago and has been sufficiently demonstrated for the predominating ions to a certain limit. Thus sea water varies only as to the degree of dilution and therefore the physical properties vary as the dilution, temperature, and pressure remain constant. One of us, in a recent paper, made the following statement:

> While the constancy of composition of some of the major constituents of sea water has been demonstrated within certain limits, it must be borne in mind that the sea is a system of tremendous complexity. Vast numbers of actions and reactions of a physical, chemical and biological nature are continually taking place. Substances in suspension and in solution are continually being poured into the sea. All the myriad forms of plant and animal life bring about a complicated series of changes. The formation and melting of sea ice, atmospheric effects, volcanic activity, and slow geologic processes, all exert an influence upon the composition of the waters of the seas. In spite of the vastness and complexity of innumerable factors, a point of balance or equilibrium persists. The resultant of any effect when compared with the entire mass of water may be very small, hardly detectable for the predominating ions, but readily measurable in many instances for those occurring only in very low concentrations. Thus an increase or decrease of 0.1 milligrams per liter of sulfate ion in a water containing 2,500 milligrams per liter of the same ion could hardly be measured, but an increase or decrease of the same amount of the phosphate or ammonium ions in sea water would readily be measured and would undoubtedly be described as a most decided change. In other words, the predominating ions in sea water appear to be present in quantities that are definitely proportional to one another, but those existing in very small concentrations tend to vary materially according to conditions.

In the light of the above statements these substances of small concentrations present a most fascinating and fruitful field for study and one that will throw much light upon the various processes taking place in the sea. Up to the present time, variations in phosphates, nitrates and silicates have occupied the attention of many investigators, and much useful information has resulted. However, the presence and variation of a long list of other substances occurring only in minute traces have been practically ignored, due solely to lack of reliable and consistent methods of analysis. In some of the European waters the

limiting factor for the mechanism of certain biological processes has been shown to be due to phosphates and nitrates, but Atkins concludes that silicates are not a limiting factor for these waters. The general experience on the Pacific Coast of North America has been that none of these substances are limiting factors, except in certain estuaries, because of the marked evidences of upwelling from the ocean depths. The rôle played by iodine, fluorine, lithium, strontium, manganese, iron, copper, etc., and the extent to which they actually occur in sea water is unknown. In many instances the situation is complicated by the presence of organic matter. This organic material undoubtedly contains the secondary alcohol group and under such circumstances many of the metallic ions react with it to form complexes and therefore direct analysis is defied.

In many of the studies of sea water very little distinction has been made concerning the nature of the various waters, and the universal custom has been to designate the differences almost solely on differences in salinity. While true ocean water is of constant composition, varying only as to the dilution, accurate studies of ionic ratios show that there may be very slight differences between the waters of the several oceans. However, it has been shown in the laboratories of the University of Washington that with more diluted waters, such as found in the Baltic Sea, at the mouths of rivers and in certain stagnant areas, the ionic ratios may show considerable variation. The diluting waters will invariably cause an increase in the ionic ratios unless there is considerable adsorption by the suspended particles in the fresh water as they come in contact with the sea water or marked evidence of bacterial action in the brackish water. Thorough detail studies of the physical and chemical phenomena of brackish waters, such as found at the mouths of rivers which deposit waters unpolluted by industrial wastes, will give most important information. Such data would be invaluable for comparison with conditions where the diluting water is highly polluted, and point to possible remedies and regulations that must be made in regions where the seashore is densely populated over a wide area and where there is marked industrial activity. The detail chemistry of the waters of lagoons, of oyster beds, salt marshes, long narrow estuaries where there is little complete flushing of water, etc., all offer most interesting problems, but the average oceanographical chemist, working by himself, is appalled by the vast amount of very accurate data that must be accumulated before he is justified in drawing conclusions.

Whether the great biological activity along the coast lines is due to physical, chemical or biological conditions, or a combination of all three factors, can not be stated, but the chemist can certainly supply data that would aid in the solution of this most complex problem.

The study of the presence of dissolved or chemically combined gases is a field that has attracted many investigators. Due to the simplicity, accuracy and ease of duplication of results, Winkler's method for the determination of dissolved oxygen has resulted in the collection of much useful data from which far reaching conclusions have been drawn. However, the rather unusual and interesting results in the study of the distribution of dissolved oxygen obtained on the last cruise of the *Carnegie,* indicate that there is a great deal that is yet unknown. In fact it might be better to say that this study of the *Carnegie* has initiated a series of perplexing problems as to the distribution and circulation of dissolved oxygen in the oceans proper.

Unfortunately, the study of dissolved nitrogen has been seriously neglected,

largely due to the difficulty of the methods of the determination, particularly on board ship. To obtain a knowledge of the nitrogen cycle in sea water, many determinations of dissolved nitrogen are imperative. The possibility of nitrogen fixing bacteria in the sea has been hinted by several authorities, and probably one of the best methods of the demonstration of the existence of such bacteria would be through studies of the distribution of dissolved nitrogen.

Like dissolved oxygen, the presence of free carbon dioxide, bicarbonate, and carbonate ions has received considerable attention, but much of the most outstanding work on this subject has come from results based on purely laboratory conditions. The relationship between pH, dissolved oxygen, carbon dioxide and total alkalinity has been shown to hold rather exactly for closed systems in which all conditions may be controlled, but when these data are applied to the sea itself, situations are ofttimes encountered which do not harmonize in any way with those that would be predicted from laboratory data. Whether one is warranted to make certain calculations dealing with the different phases of carbon dioxide equilibria on a purely physical chemical basis seems to be open to question, but perhaps all conditions have not yet been taken into account.

THE METAMORPHOSIS OF THE HORSE DOCTOR

By T. SWANN HARDING

MT. RAINIER, MARYLAND

If a man gets sick in this country to-day there is no sure way for him to procure the services of a conscientious and competent medical doctor. It is, therefore, a curious fact that if a man had a sick horse to-day, he would not be compelled to wonder where he might obtain the services of a good veterinarian. He could wire the Bureau of Animal Industry at Washington and find out the approved or the accredited veterinarians in his vicinity and be sure when he selected one of them that he had a well-trained and competent man in charge of his horse. It is furthermore a fact that, so well organized has veterinary medicine become in recent years, there actually exist less and less diseases for horses, cows, hogs and other lower animals to contract. For veterinary medicine has gone in the business of producing animal health *en masse;* it has very largely substituted wholesale for retail methods. Without sacrificing anything of real value in the individualism we all cherish so much and understand so little, it has stimulated hundreds of scientists of various sorts to work together for the good of animal health, and its accomplishments are such as to bring pause to human medicine.

For, come to think of it, what ever became of the old-fashioned veterinarian? The old fellow had a gig, a knife, a needle, a ball of twine and a few other instruments he was continually getting mixed up with the carpenter's tools. He rode round the countryside cutting hogs, opening abscesses, or visiting individual horses, hogs and cows in the manner medical doctors visit to this day. How many of him did we have then and how many now? How many veterinary schools were there then, and what sort of education did they provide? How about now? How many veterinarians does the Bureau of Animal Industry now employ and what has this bureau accomplished? What part has it had in enabling the veterinarian to combat animal disease in wholesale fashion? The average human doctor, even to-day, regards the veterinarian as an inferior sort of animal somewhat lower than a dentist but perhaps on the mental level of an osteopath or a chiropodist. What reason has he for this attitude? Is the human medical profession even as well organized as veterinary medicine to-day? Hasn't veterinary research actually made important contributions to human medicine? Isn't the veterinarian worthy of more respect now? In short, hasn't he shed his horse-doctor integument to become a serious scientist whose work the human physician might rather emulate than deprecate? Let us see.

Many people are under the impression that the horse doctor is as extinct as the horse. Not so at all. Neither animal is extinct. Raymond A. Pearson[1] assures us that the veterinarian is not passing. He also reminds us that we owe the existence of the Panama Canal to veterinary medicine, because Smith and Kilborne's solution of the Texas tick-fever problem supplied the technique for the control of yellow fever —and there you are.

[1] *Journal of the American Veterinary Medical Association,* October, 1927.

David S. White, dean of the college of veterinary medicine, at Ohio State University, remarks,[2] however, that while 85 per cent. of our medical graduates to-day have baccalaureate degrees, only 2 per cent. of our veterinarians possess them. Furthermore, there are twice as many students in our medical schools as there are practicing veterinarians in the entire United States. However, low entrance requirements and quick courses for veterinarians are passing.

In 1890 Dean White went to Europe for three years' study and he returned to find a former vacant lot occupied by a school of veterinary medicine with 400 students attending, and which had already sent forth one entire graduating class! To-day, we have but a dozen recognized schools of veterinary medicine in the country, with a total of some 700 students, and not more than a few over 100 students at the largest of them. Germany, which is the size of Texas and has about one third our animal wealth and one half our population, has five well-equipped schools of veterinary medicine; France, with one third our population and one fourth our animals, has three schools; and Great Britain, with 40,000,000 population and one fourth our livestock, has four. The total livestock of all these countries combined amounts to one half our horses, two thirds our cattle, and three fifths our swine or sheep, yet these countries have as many schools of veterinary medicine as we do and double the number of practicing veterinarians. This indicates that veterinary medicine has been a little too much neglected in America in view of rational, scientific requirements.

An editorial in *The Prairie Farmer*, October 12, 1929, says:

> It is reported from the Kansas Agricultural College that the enrolment in the veterinary courses this year is higher than for many years back. This is encouraging, for a real and serious shortage of competent veterinarians is imminent in the near future unless more young men of the right kind do decide to fit themselves for the practice of veterinary medicine. Most people do not realize the importance of the veterinarian in maintaining the health and well-being of our farm animals, and still less do they realize how much the health of the human race depends on the health of the beasts. Rabies, anthrax, and glanders, to say nothing of tuberculosis, are all terrible diseases that are easily communicated to man from the lower animals. While it is a fact that the spread of knowledge of sanitation and animal hygiene among the farmers will do much to head off animal disease, nevertheless, it is still true that the brunt of the battle must be borne by the men who are trained specifically for the task—in other words, the veterinarians. The lack of trained men opens the way for the quack and the vender of nostrums. As the country grows older and the number of animals increases, the problem of disease and parasite control becomes more serious—that is a plain fact that cannot be controverted. Twenty-five years from now we will consider ourselves fortunate if we have a sufficient force of veterinarians adequately trained for the task that will be theirs.

According to an article in the August 25, 1927, *Hoard's Dairyman* on "The Veterinary Situation," by D. S. Burch, veterinary medicine arose in France in 1762 with the object of training men to save war horses. The first college was established in the United States in New York in 1857. To-day all our private veterinary colleges are closed for lack of students, and only full-time, four-year-course state colleges remain. Three thousand students enrolled in 1916; 582 in 1926; the 132 graduates in 1926 would not fill Bureau of Animal Industry vacancies alone, as it employs 1,350 veterinarians with an annual 10 per cent. turnover. When hog cholera ran amuck in September, 1926, serum could not be made fast enough and one and five tenths million hogs died unnecessarily because serum could not be prepared in less than two months. The veterinary shortage is even more severe

[2] *Journal of the American Veterinary Medical Association*, February, 1929.

than the serum shortage as it takes four years to make one veterinarian! Yet the field for well-trained veterinarians has broadened greatly and includes disease control, city milk and meat inspection, state and county veterinarians, the manufacture of biologicals, promotion of stock raising, army work, teaching, equine work on 17 million horses, tuberculosis testing, fur farming with valuable breeding stock, and the poultry industry. There are now only 10,000 veterinarians in the United States, or scarcely one for each 18,000 domestic animals, not counting dogs, cats, fur bearers, or poultry, and many of the number are perhaps near retirement or are already partially inactivated.

Each year the report of the chief of the Bureau of Animal Industry calls attention to this situation and the bureau has been most active in fostering veterinary education. In 1927 the number of colleges whose graduates were eligible to compete in United States Civil Service examinations was 12. The freshmen registered were 193 as against 165 the previous year. A marked shortage of veterinarians for over 30 years was deplored. In 1928 the number of accredited veterinary colleges remained 12, but 242 freshmen were enrolled, there were 136 graduates, and "a growing recognition of the veterinary profession as a promising career for young men" was presaged.

The fact should be emphasized that the United States Government, through the Bureau of Animal Industry, without being autocratic, and without any unpleasantness, has taken stock of the situation, and has aided materially in weeding out low-grade colleges. The entrance standards and educational requirements of colleges have been raised, and the country now has well-trained and well-equipped veterinarians. Finally, the Bureau of Animal Industry has so successfully advertised the shortage of veterinarians that increased enrolments are now observed and we shall soon have as many highly trained, accredited veterinarians as we need—considering the manner in which the combat against animal disease is organized. It is, therefore, quite possible for a government agency skilfully, quickly and efficiently to renovate a profession in all its branches, and thus to render the public a tremendous service. If some such overhauling could be carried out in the profession of human medicine, most of the pressing problems which so agitate and beset us in this respect would automatically disappear.

For, in spite of depleted numbers, our veterinarians care for a tremendous animal population in what must be a very efficient manner. The domestic animal population was one and one third times our human population, or 183 million, in 1922, according to Dr. John R. Mohler.[3] According to the 1920 census our horse and mule population was 27,283,413, and 17 to 19 million animals were regularly needed for draft work in crop production, while 2 to 3 million more were required in non-agricultural work. According to Pearson,[4] the value of livestock on American farms was 3 billion dollars in 1900 and 5 billion in 1926, the latter being a low figure due to temporary conditions. There had been no decrease in the number of horses and there were more horses on our farms in 1910 than in 1900, and in 1920 than in 1910. Mules had increased in number rapidly. Besides that we slaughter some 115 million animals annually which must be examined for disease, and cows, beef cattle, sheep, swine and small animal pets abound. Pearson said there were 21,532,000 horses and mules on American farms in

[3] *Journal of the American Veterinary Medical Association*, 62, 592.

[4] *Journal of the American Veterinary Medical Association*, July, 1927.

1900 and 22,312,000 in 1925.[5] Finally, the U. S. Department of Agriculture Circular 241, which contains a livestock census in tabular form from 1850 to 1929, notes a steady increase in our cattle, sheep and swine population.

The disease problem confronting the veterinarian bifurcates into two distinct parts: (1) The fight against animal diseases *per se*, and (2) the eradication of animal diseases which can readily attack human beings as well. Hence veterinary medicine is closely related to human medicine. The toll from animal disease may total $200,000,000 annually from stock loss alone. The country must be and is efficiently protected from invasion by foreign animal diseases, such as contagious pleuropneumonia, rinderpest, surra, Malta fever and the foot-and-mouth disease. When dourine (an equine venereal disease) menaced the horses of ranches in the Dakotas, Montana, Wyoming, New Mexico and Arizona, and control measures proved unavailing, a complement-fixation test for its detection was developed in 1912 and the disease was thereafter rapidly eliminated. Hog cholera has scourged American swine for ninety years, but a serum, also developed in the Bureau of Animal Industry, in 1905, gives temporary immunization and that, plus virus, gives permanent immunization which, together with quarantine and sanitation, will cut the losses almost to nil. Cooperative tick eradication began in 1905 and must proceed to the last tick, which will thus banish Texas fever. Investigation continues on bovine infectious abortion and as soon as the bureau's general staff has its method ready, the disease will disappear. More rapid tests for the detection of animal tuberculosis have been developed and infected animals are rapidly being eliminated from herds.

A calamity like the foot-and-mouth disease demands the wholesale destruction of herds of valuable cattle. Congress grants an indemnity for herds destroyed, but this is not legally compulsory; it is a mere gratuity, the destruction of the sick animals being legally regarded as the abolition of a nuisance. Often 100 per cent. eradication of contagious animal diseases has been accomplished. Glanders is now a curiosity; dourine is nearly gone; Texas fever is greatly diminished; scabies is controlled, and lip and mouth ulcers no longer menace sheep; "hollow-horn" and "wolf in the tail" vanished some time ago.

The fundamental research performed by the Bureau of Animal Industry in 1888–1893 which demonstrated that a microorganism found in the blood of cattle affected with Texas fever is the actual cause of the disease, and that the cattle tick was the intermediary host, was the first demonstration that a microbial disease could be transmitted exclusively through the agency of an intermediary host or carrier. This discovery ranks with the foremost achievements of medical science, for it led to the knowledge that yellow fever, malaria, typhus fever, African sleeping sickness, Rocky Mountain fever, nagana and other such infections were carried by an intermediate host. The development of the complement-fixation test used in dourine led to further discoveries in this line in the investigation of botulinus toxin and cerebrospinal meningitis in horses. It also led to the

[5] The following table given in one of the most recent of the Department of Agriculture publications indicates, however, that very recent estimates show a decline in the horse and mule population.

Horses and mules	1920	1929
On farms	25,323,000	19,506,000
In cities	2,084,000	1,450,000
Totals	27,407,000	20,950,000

This is a decrease of 6,457,000, or 23 per cent.

development of an immunizing agent against hemorrhagic septicemia, or sleeping fever, of cattle, a biological product now prepared commercially, which has proved potency and has saved livestock men enormous sums of money. The Bureau's discovery of a new species of hookworm prevalent in the Southern States led eventually to the establishment of the International Health Board of the Rockefeller Foundation, and the Bureau's subsequent introduction of carbon tetrachloride as a remedy for hookworm disease has resulted in the treatment therewith of 15 million cases throughout the world. This, too, we owe to governmentally financed veterinary medicine. Research on the intestinal roundworm of swine led to a new system of hog raising based on protection from roundworm infestation and a saving of a million a year in the Corn Belt alone.

Each annual report of the Chief of the Bureau of Animal Industry notes the vigilant and effective pursuit of this scientific work. In 1927, for instance, 347 counties were observed to be free from bovine tuberculosis; a survey of animal parasites was undertaken; there was a record production of hog-cholera serum which checked serious outbreaks of that disease; many more counties were announced free of Texas ticks; research progressed as usual. In 1928, the report observed that 11,300,000 cattle were tuberculin tested; further counties were released in the tick eradication work; supervision over licensed establishments producing animal biological remedies was successfully continued; the country was protected against a dangerous outbreak of foot-and-mouth disease; progress was reported in work on infectious abortion, which exacts an annual toll of $50,000,000; animal parasite investigations were continued and the problem of anaplasmosis was attacked.

As we have already seen, and as J. Howard Beard reminds us, "The Veterinarian Contributes to Public Health."[6] For man also is susceptible to tuberculosis, anthrax, Malta fever, rabies, glanders, pyogenic infections, foot-and-mouth disease, diphtheria and trembles. Anthrax is as old as history. Moses described "a boil breaking forth with blains upon man and beast." Homer, Seneca and Virgil mention the disease. In 1849 the human and animal types thereof were found to be identical; we owe much to the later work of Pollender, Koch and Pasteur. The first case reported in America was in Philadelphia in 1834; Louisiana suffered anthrax epidemics in 1835 and 1851 and cases had occurred in every state by 1900. There were eighteen cases with nine fatalities in New York City in 1919–1920. Men handle animal products and easily become spore infected. Trichina, one of the roundworms of rats and hogs, causes gastro-intestinal trouble in man with fever and muscular pains.

Naturally, therefore, veterinary research is valuable in combatting human diseases. The study of splenic fever and the cattle-tick-born fever helped us understand Rocky Mountain spotted fever, relapsing and South African tick fever in man. The investigation of surra in horses and nagana in cattle offered suggestions for the treatment of trypanosomiasis in man. Pasteur's immunization of chickens against cholera and of cattle and sheep against anthrax was a forerunner of our human typhoid, cholera and plague inoculations. The study of malnutrition in livestock definitely assisted in the study of human pellagra, scurvy, rickets, beriberi and the other metabolic diseases. Tumors in animals have been studied to the advantage of cancer research. Treating the young animal helps the

[6] *Nation's Health*, June, 1921.

pediatrician while the obstetrician learns from abnormal parturition and *post partum* illness in animals. In his "Animal Reservoirs of Human Diseases,"[7] Theobald Smith went very thoroughly into this subject and mentioned the following diseases as transmissible from animal to man: glanders, bubonic and pneumonic plague, Malta or undulant fever, tularemia, rat-bite fever, infectious jaundice or Weil's disease, anthrax, rabies, paratyphoid diseases, tuberculosis, Rocky Mountain spotted fever, septic sore throat, besides other pathological conditions caused by higher animal parasites.

In all this work the Bureau of Animal Industry may be regarded as a sort of general staff headquarters. Its activities are not autocratic, nor do they deny the opportunity for individual service among private practitioners of veterinary medicine. Through its ability to establish quarantine for the control of animal diseases the bureau has, of course, some leverage in cases where state cooperation might be dilatory, but local cooperation is usually excellent, though the bureau's leadership in planning and conducting disease-control work from a national standpoint can not be denied. Policies regarding livestock-sanitary problems are formulated for the most part at conferences of the United States Livestock Sanitary Association in the affairs of which bureau officials have long been active, and the decisions of the organization are quite uniformly in accord with bureau recommendations.

The Bureau of Animal Industry in Washington is so organized that the well-correlated efforts of every type of research scientist indicated are coordinated for the one end of animal disease eradication. Here is a case where highly organized research, directed at a very definite end, accomplishes theoretical results and practical applications of fundamental importance. Furthermore, the bureau maintains a list of accredited and another of approved veterinarians. These men are private practitioners who, by examination or otherwise, have demonstrated their ability to perform certain classes of work to the satisfaction of bureau standards. An accredited veterinarian is a higher grade man than an approved veterinarian, and any citizen, by writing to the bureau, may learn the names of the accredited and approved veterinarians in his locality.

It is, of course, still true that some old veterinarians who are little better than charlatans remain in practice, but the type is rapidly dying out and is being replaced with highly qualified men. It is also true that a horse might yet manage to die unnecessarily of some ailment which was not too contagious nor infectious, provided his owner could not afford to pay an accredited or an approved veterinarian. But the real point to be considered is that while human beings are still exposed to really preventable diseases like malaria, tetanus and diphtheria, and while human medical research is sporadic, ill-coordinated and frequently involves shameless duplication of effort, so thoroughly has veterinary medicine been organized that the animal is well protected against any pathological condition preventive medicine can eradicate.

Of course, a disease like foot-and-mouth, which is widely prevalent elsewhere in the world, must occasionally get into this country by chance. That has happened nine times in the past 60 years, and it happened in California in January, 1929. That outbreak was completely eradicated two months after its discovery. Why? Because the Bureau of Animal Industry has an es-

[7] *Bulletin of the New York Academy of Medicine*, 1928, Vol. iv, 476–496.

tablished method of combatting such outbreaks and maintains a trained and experienced force of foot-and-mouth disease fighters that is not equalled in any other country. The government ably finances this work and no time is lost when an outbreak occurs.[8]

Consider the 1929 outbreak. It originated in Los Angeles County. On October 6, 1928, the S. S. *City of Los Angeles* left San Pedro for a South and Central American cruise. On November 2 she took on board approximately 18,000 pounds of fresh meat at Buenos Aires. She still had some of this on hand when she reached Wilmington, California, on December 8, 1928, and on December 20 transferred some of it to the S. S. *Caliwaii*. This ship sailed for Honolulu on December 22 and the *Los Angeles* followed her on December 29. Both ships used up all their South American meat before returning to port January 12 and 18, 1929, but the trimmings from this meat were transferred to garbage cans along with other garbage and discharged from the ship; the garbage, infected with foot-and-mouth disease from the Argentinian meat, was steamed, collected for the San Pedro Commercial Company and later sold to the owners of two large hog ranches—Mr. Frank B. Haas and Russell and Knight. Foot-and-mouth disease appeared on the Haas ranch only.

Mr. Haas first noticed lame hogs on January 10. This might be attributed to broken crockery, but on January 11 there were other lame hogs, and on Sunday, January 13, still more lame hogs were found in an adjoining pen. Mr. Haas now tried to get in touch with the county department of livestock inspection by phone, but failed; he left word for Dr. Conti of this inspection service to visit his ranch on the fourteenth. Dr. Conti examined the animals, made confirmatory inoculations of other animals, quarantined the premises, and notified Dr. Snyder, the Bureau of Animal Industry cooperative inspector in the district. On January 16, Dr. Snyder wired the bureau at Washington that he believed the hogs had foot-and-mouth disease; on the seventeenth, inoculation tests proved positive; federal quarantine was established on the nineteenth, and all infected and exposed animals were slaughtered and buried at once.

For a while thereafter no new cases appeared. On January 30, however, a nearby herd of 51 dairy cattle was found to be infected, was appraised, slaughtered and buried. On February 2 another nearby dairy herd of 75 developed symptoms and was disposed of that night. A fourth herd, composed of 33 dairy cattle, was discovered infected at noon, February 6, and was appraised, slaughtered and buried that afternoon. The fifth and last herd was found infected on February 16; it was composed of 67 animals and was buried without military honors the same day. No further signs of the disease appeared and quarantine was lifted March 18. The quarantined premises were each immediately surrounded with high fences of chicken wire, which barriers excluded dogs, cats and poultry, and also gave assurance that hay, manure and other possible carriers of the disease were not removed from the premises.

The method was drastic and sanguinary, but it was effective. The country was protected from one of the most dangerous cattle diseases in existence. The personnel in charge is thoroughly trained, and individuals moved to the focal point as directed from Washington, and performed their duties in a business-like manner. Walking in, killing a man's valuable cattle, paying him the appraised price

[8] J. R. Mohler, *Journal of the American Veterinary Medical Association*, September, 1929.

the government grants, digging a trench with a machine, casting in the bodies, ripping up the hides and covering the entire mess of meat with lime both invades his domain and smacks him squarely in the face with a large gob of state socialism. But it represents organized effort acting effectively in a social emergency and it serves its purpose. In the case of smallpox, however, it is difficult for the authorities to enforce vaccination upon individuals who resent such interference with their "personal liberties." In the case of certain other diseases among humans, eradication would be fairly easy if physicians but conscientiously did their part. Finally, the fundamental tenet of economically organized medicine in America to-day is absolutely fanatical individualism, and the slightest tendency towards soundly organized efforts to control disease on a national basis is denounced by the journals of the medical guild as "pauperizing" the public or as state socialism.

It is, of course, very much easier to eradicate animal diseases than to wipe out human ailments because, if you can't do anything else, you can kill a few hundred patients. This does not materially affect the major thesis of the present paper, *i.e.*, that veterinary medicine has been so efficiently organized to combat animal diseases that the result has been the complete eradication of some diseases and very serious inroads upon the disastrous effects of others; furthermore, that within a very short period of time, the status of the veterinary profession has been raised and effective means have been devised to use without delay the latest findings of an organized body of scientists of various sorts, engaged in fundamental research, in a practical way, to fight diseases among animals.

In short, a sick or indisposed animal is regarded as an acute social problem about which the community, through "socialistic" (if you please) governmental agencies is prepared to take action. But a sick man is not at all sure of such certain expert care—at least not so long as experts insistently inform us that preventable deaths cost billions of dollars annually in America to-day.[9]

[9] This article was personally corrected and endorsed by Dr. John R. Mohler, chief of the United States Bureau of Animal Industry in the Department of Agriculture, in so far as it is concerned with the activities of that bureau.

SCIENCE SERVICE RADIO TALKS

PRESENTED OVER THE COLUMBIA BROADCASTING SYSTEM

THE RÔLE OF CELLULOSE IN INDUSTRY

By Dr. HENRY K. BENSON

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THE instinct to keep a diary is well-nigh universal, even in ancient times. The materials used for writing at first were those that required but little mechanical fashioning to fit them for that purpose. The love letters and patriotic orations of ancient times were engraved on flat stones or were impressed in clay which was afterwards dried or hardened by sun or fire as in the Babylonian tablets. Thin boards of wood covered with wax and plates of ivory or metal have been used. The skins and intestines of animals have also been made fit for writing upon; and there still exists in the Sorbonne in Paris a manuscript certified by an ancient librarian to be written upon human skin as well as an elegant copy of the Bible which the Abbe Rive believed to be on the skin of a woman. Little wonder that it should have been a beautiful edition.

When the Egyptian papyrus was introduced, all these things fell into disuse except the parchment made from the skins of sheep and young goats, which is still preferred for certain purposes such as college diplomas and other rarely used documents. Quite appropriately, the parchment made from the skins of old goats is used for drums and tambourines.

The nearest resemblance to modern paper was that made in Egypt from the papyrus plant. This plant has a soft cellular flower stem ten to twenty feet high, of a triangular shape and made up of thin coats of plant tissue. These thin coats were separated by means of pointed mussel shells, spread on a table sprinkled with Nile water in such form as the sheets required and washed over with the same. On the first layer of these slips, a second was placed crosswise so as to form a sheet of convenient thickness, which after being pressed and dried in the sun, was polished by means of a shell. The paper metropolis of Egypt was Alexandria, which supplied Europe and Asia for several centuries and acquired immense riches thereby. So profitable was the business that one individual boasted that the revenue from the paper in his possession would maintain a large army.

We are now making paper from fibrous material floating or suspended in water. For the discovery of this art some 1,800 years ago, we are indebted to the Chinese. They used the inner bark of the bamboo and mulberry tree as well as hempen rags. The pulp obtained was made into paper on a frame of fine reeds, pressed under large stones and dried by fastening the sheets on the walls of a room. The Chinese also made paper from cotton and linen rags and from rice straw. Their art was evidently transmitted to the Hindoos and the Arabians, who carried it into Spain, where, after the expulsion of the Moors, it spread into the other countries in Europe. In the latter part of the fourteenth century, linen paper was made in England, and in 1712 Peter the Great of Russia was so much pleased with what he saw in a paper mill at Dresden that he engaged paper makers whom he sent to Moscow to establish a paper mill at his own expense.

The manufacture of paper in the American colonies was early established, the first plant being that of William Rittinghuysen near Philadelphia, in 1690. Apprentices from this mill became the pioneers of the American paper industry. The raw material in this country was rags, and purchasing agents in those days had the skill and culture of modern sales agents. In the *Boston News Letter* of 1769, it was announced that "the bellcart will go thru Boston before the end of next month to collect rags for the paper mill at Milton," and to aid the public zeal these lines were appended:

Rags are as beauties, which concealed lie,
But when in paper how it charms the eye;
Pray save your rags, new beauties to discover
For paper, truly, everyone's a lover;
By the pen and press such knowledge is displayed,
As wouldn't exist if paper were not made.
Wisdom of things, mysterious, divine,
Illustriously doth on paper shine.

And again we listen to a masterly appeal in New York:

Save your rags! This exclamation is particularly addressed to the ladies both young, old, and middle-aged, throughout the northern part of this state by the subscribers who have created a paper mill in the town of Moreau, near Fort Edward—nor is it thought that this appeal to our fair countrywomen will prove unavailing when they reflect that without their assistance they can not be supplied with the useful article of paper. If the necessary stock is denied paper mills, young maids must languish in vain for tender epistles from their respective swains; bachelors may be reduced to the necessity of a personal attendance upon the fair, when a written communication would be an excellent substitute. For clean cotton and linen rags, of every color and description, matrons can be furnished with Bibles, spectacles and snuff; mothers with grammars, spelling books and primers for their children; and young misses may be supplied with bonnets, ribbons and earrings, for the decoration of their persons (by means of which they may obtain husbands) or by sending them to the said mill they may receive the cash.

I have drawn on the history of paper making to show you the slow transition of the centuries from the papyrus of the Egyptians to the fibers obtained from the cast-off clothing of the population of the whole world. In a book dealing with the progress of paper making, published in 1876, the author says that although experiments under way at that time were so successful as to lead many to hope for an economical mode of converting the forests into paper to supply the all-devouring maw of the press, yet "it is still thought that we shall never find anything to answer the purpose so well as linen and cotton rags."

To-day this forecast no longer applies. We have turned from rags to the ever-renewing and perpetual forest mantle of the earth for our supply of fibers, and through our knowledge of chemistry we have learned to utilize the giants of the forest for the printed pages of our culture. The tree has taken on an unwonted reverence. In kindred sciences we have learned its life history. We know that the magic of sunlight transforms the moisture of the soil and the carbon dioxide of the air into cellular structures akin to starches and sugars, and to protect them against the storms of centuries, nature has insulated these fibers by thickening them and coating them with a highly resistant material to which we give the name of lignin.

The problem that confronted the paper industry fifty years ago was to find a solvent for this lignin coating. The answer came in the processes that utilized caustic soda and sulfurous acid to dissolve the lignin and thus release the fibers of cellulose and so to interweave them in thin layers as to constitute the paper sheets which we now use so extensively and upon which we are so dependent for the spread of knowledge and extension of culture.

The future requirements of pulp and paper are difficult to forecast. Shoes, socks and shirts are sometimes made from paper. We try to drink coffee or

cocoa from paraffined cups. The sanitarian has banished the old towel and makes us blot our faces instead of wiping them. The fruit industry wraps its products in tissue, the poultry industry packs its eggs in cardboard trays, the dairy industry seals its butter in paper cartons, the cement industry delivers cement in paper bags; even explosive materials used in blasting are shipped in paper bags, ready for the farmer to mix and blast out a stump. The interiors of houses are often largely constructed of wood pulp; it is only necessary to apply some waterproof coatings which we have in order to use wood pulp for the exteriors as well, and as the per capita consumption of lumber diminishes that of pulp and paper increases. It is not too much to expect that soon we shall sail the high seas in ships of paper, fly through the air upon wings of paper, and dwell in houses of paper.

Spectacular as this forecast of the future uses of cellulose in pulp and paper may be, yet more striking are the uses to which it has been put in chemical derivatives. I have said that chemically it is related to sugar and starch. In fact, it may rather readily be converted into sugar, and this has been done in Wisconsin and the cows that fed upon this food gave increased milk with higher butter fat than when fed upon barley. In South Carolina the Du Pont Company some years ago used the sawdust from three sawmills for treatment by dilute acid and fermenting the resulting sugar to form the ordinary grain alcohol. As the late Dr. Slosson used to say, "It is now possible for a man after he has read his morning paper to get drunk on it."

More intimately associated with our daily life is the cellulose product which, under the name of artificial silk or leather or "Duco," pleases us so much and which constituted a very considerable portion of the Christmas which we have just passed. Until recently it was believed by chemists and manufacturers that only cotton cellulose could be successfully nitrated, but the Germans demonstrated very plainly that the smokeless powder which they made from wood pulp cellulose carried a projectile an even greater distance than cotton cellulose, and soon to an increasing extent we shall use wood cellulose for the great industries that are rising up before our eyes.

When properly manufactured wood pulp is treated with acid, a new product known as nitro-cellulose or cellulose acetate is obtained; or when treated with an alkali and carbon disulfide, a solution of cellulose is obtained. If this solution is squirted out of tubes through extremely minute holes into acidified water, each tiny stream becomes instantly solidified into a silky thread which may be spun and woven like that ejected from the spinneret of the silkworm. The wonderful development of fiber silks, celanese silks and other artificial silks, is well known to this radio audience, and I hope this excursion into chemical industry will not detract from the pleasure of wearing them. History tells us that at one time fig leaves were prominent articles of attire, but now we have advanced to the place where we make use of the whole tree in order to be properly clothed in silken apparel.

Not only are we dependent upon cellulose for the decorative effect of our clothing, but our automobiles have greatly improved their appearance under a glossy coat. Formerly the pride of the auto salesman consisted in telling his prospect of the eight or ten coats of oils and gums which had been carefully brushed and rubbed over the surfaces and how finally these coats were baked in an oven. Now we take cellulose, nitrate it and dissolve it in a quick-drying liquid, add some coloring,

and after the painter has put on his gas mask, he quickly sprays the solution of cellulose and pigment upon the properly prepared surface of the automobile. In a few hours it will have dried without giving a chance for the dust and millions of flying gnats of the atmosphere to settle on it. Or the solution may be spread upon a cellulose fabric such as canvas to make a substitute for leather, which we use for auto tops and curtains, or for elegant flexible book binding, or to cover the furniture of the drawing room. Even shoes are now made which, as Dr. Slosson used to say, a vegetarian could wear without pain of conscience, for the soles are made from the sap of the rubber plant and the uppers of cellulose-coated fabrics. The live-stock industry will soon have to do double-page advertising in the magazines to inform coming generations of the virtues of leather.

These cellulose solutions are making chemists out of every one. The butcher has to ask for viscose tubes for sausage containers; the candy manufacturer buys transparent viscose film under the name of cellophane with which to wrap up his candies; the trunk manufacturer knows that vulcanized fiber made by running sheets of paper through a solution of a zinc salt makes trunks of good quality; and the motion picture theater advertises that acetate films used by them are non-inflammable. These chemical terms are being incorporated into the street language of the day.

I can not conclude without a moment's tribute to my colleagues in science who have made these things come to pass. Because they were interested in searching for new facts, they penetrated beyond the borders of the known and laid open to our gaze these results of their explorations. We have quickly taken them and are now using them and enjoying them.

THE EVER-CHANGING LANDSCAPE

By Dr. HENRY C. COWLES

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WHEN one surveys a majestic forest, with trees several centuries old, one may very likely think that such a landscape is eternal. One speaks of the "everlasting hills"; but geologists have proven that hills are really ephemeral, and by no means everlasting,—though hills usually are ephemeral in a geological sense only, rather than in terms of human life.

Forests, on the other hand, are much shorter-lived than hills. In many cases the human life is long enough to see marked change in the natural plant covering of our hills and valleys. Doubtless many of you have seen a forest cut down and another one, quite different, take its place.

Similar changes take place without human influence, though much more slowly. A pine forest in our northern states, if it has any undergrowth at all, is likely to have an undergrowth of hardwoods. When the forest is cut down the hardwood undergrowth is left free to grow to maturity and thus we have the phenomenon of a hardwood forest replacing a pine forest. The same thing would happen in time if the pine were not cut. Eventually the pines would die of old age and the hardwood undergrowth would then come into its own.

Very different is the situation if the pines are so densely crowded that the shade is too great for undergrowth. When such a forest is cut it is likely to be replaced by aspen and white birch,

and the problem at once arises: Whence came the seeds to supply the totally new growth?

Some have thought that seeds can live in the ground for centuries, springing into activity when the chance lies open; but experiments have shown that the seeds of these trees soon lose their vitality. Aspen seeds in particular live only a few days. The cause of replacement of pines by birch and aspen is along quite different lines. Aspens and birches are very common trees in our northern states, and every year they produce their seeds in countless millions.

What is quite as important, the seeds of these trees are scattered easily and widely by wind. Probably there is scarcely a foot of ground in the aspen and birch country which is not supplied every year by seeds of these trees. In most instances these seeds come to nought, but when they happen to fall in areas that have been cut over or burned, they germinate and produce forests.

The foregoing illustrates one of the commonest and most wide-spread of natural occurrences, namely, the phenomenon of plant succession. Plant landscapes are, for the most part, in a process of continual change. Some of these changes are rapid enough for one to get a photographic record of them over a period of years; while others are so slow that a human lifetime scarcely suffices for the observation of much change.

The sand dunes of our lake and ocean shores are among the most dynamic of such landscapes. Even on a single windy day a dune may change its place and form considerably. One of the most interesting cases of the struggle for existence is to be seen in the dunes, where some of our hardiest plants have a bitter fight for life against the sand. Trees like the cottonwood, shrubs like the willow and sand cherry, and grasses like the sand reeds are most successful here, their victory over the sand being due to the fact that these plants are able to send out roots from their shoots if buried, or shoots from their roots if unburied.

Eventually these plants may become abundant enough to cover the dune and prevent its further movement, whereupon a host of plant species may start on the stabilized sand, representing a second stage in dune plant succession. In this second stage there may be juniper, pine, and other plants, varying with the climate and geographic location. These first plants of the stabilized dune are those that require sunlight and can endure dry soil and high evaporation.

As the pines and other trees of this stage grow in size, they cast more and more shade, which in time tends to cause the soil to become more moist and more suitable for shade plants and less and less suitable for pines—so, a situation arises where hardwoods such as oaks germinate in the shade of the light-requiring pines. Thus it is that hardwoods follow pines in natural succession.

Similarly, in many places the oaks may be succeeded by the still more shade-tolerant beech and maple. Since beech and maple seedlings are tolerant of the shade cast by the parent trees, they may develop in such shade without difficulty and thus carry on the same type of forest that existed there before. Beech and maple forests, therefore, may perpetuate themselves almost indefinitely. For this reason such a forest is termed a climax forest.

On clay uplands the succession is much the same as in the sand dunes but without the stress of the first stage. The end is the same and arrives more quickly. On rock uplands the later stages of the succession are the same as on sand or clay. At the outset, however, the stages are very different, the first plants com-

monly being lichens, which are extremely tolerant of sunshine and drought. These plants build up soil and prepare the way for higher plants, such as the drought-tolerant mosses, which are followed by drought-tolerant ferns and flowering plants. After a long time there may enter a stage of light-requiring trees, much like the first stage on a stabilized dune. Once in a while the succession does not go to the climax because of adverse soil conditions, as in the areas known as sand barrens. Here the succession may stop with a pine or oak stage instead of going on to the beech and maple climax.

Quite in contrast to the above, but similar in principle, is the succession of plant life in ponds. Many ponds are surrounded by symmetrical zones or belts of vegetation that may be considered dynamically as stages in succession. The first stage in a deep pond consists of aquatic plants and animals of various kinds. Upon their death their remains help to build up little by little the pond bottom, making the pond shallower from generation to generation.

After a time the pond becomes shallow enough to support such plants as water lilies. These in turn build up the pond bottom and make the pond shallow enough for such plants as bulrushes and cattails. In similar fashion these are followed by sedges and grasses and in time by trees, such as the willow, ash and elm. So there comes as a climax of pond succession a forest quite similar in nature to the climax on an upland of rock, sand or clay.

The successions noted above are such as may be seen rather generally in our northern central states. Successions similar in principle but differing widely at various points in the successional stages may be found anywhere in the country, or, for that matter, in the world. The phenomenon of succession is one of universal importance wherever plants grow, and may be looked upon as one of the most interesting of the phenomena of nature.

BIRD MIGRATION

By ALEXANDER WETMORE
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In July and August each year, after the nesting season for our birds is over and young are on the wing, flocks of martins, blackbirds, swallows, sparrows and other species gather at night to roost in some selected grove of trees or bed of rushes, and in morning fly out again over the country-side in search of food. Such roosts, when located in the shade trees of lawns or along city streets, are frequently a serious nuisance as the birds come nightly and can be driven away, if at all, only with the greatest difficulty through bombardment with guns, streams of water from fire hose, Roman candles or other means. In the case of such species as sparrows and starlings these roosts may continue throughout the winter, the only change being from grove to grove, or from trees to church towers and sheltered crevices about buildings as colder weather comes, or as the birds are disturbed by predatory owls or other enemies. With blackbirds, swallows and robins such nightly gatherings are the forerunners of seasonal migrations in which the birds concerned, with a host of other species that do not congregate in flocks, fly southward to warmer regions to remain through the period of winter. In spring they return northward once more to their nesting grounds.

Most of the birds that nest in regions

of Arctic climate, and a majority of those found in the temperate zones, perform migrations of greater or less extent, some of the species concerned making journeys that carry them from the Arctic regions to areas far south of the equator. While we think of migration ordinarily as something peculiar to the northern hemisphere there are many species of birds of the southern hemisphere that after their nesting season move north toward the equatorial regions. Even in tropical countries migrants from a distance pass through so that the naturalist working in the West Indies or in Central America may see warblers, sandpipers or other species in abundance that have come from North America.

From what has been said it appears that the phenomenon of bird migration is one that may be observed throughout the world and has from this fact been known and discussed by man from the most ancient times. Movements of birds have been correlated with the seasons, and various rites and beliefs have been coupled with them by primitive peoples. Though the migrations of birds have been recorded and studied from the days of Aristotle and other early naturalists to the present and a vast amount of information concerned with the arrival and departure of various species has been accumulated yet there is still little known of the reasons for the movements of birds as a whole. Some have attributed the underlying cause of migration to changes in food supply, others to seasonal change in temperature, to a crowding that has driven birds from their original homes to which they have an inherited desire to return for the nesting season, to a reaction to seasonal change in intensity of light, to shifting of range due to changes in the environment in past geological ages, to seasonal physiological changes in the birds themselves and so on.

After careful scrutiny of all the theories that have been proposed to account for bird migration it appears to the writer that while each may explain the movements of a few species no one theory is sufficient to cover the entire group of birds as a whole. The factor that initiates or starts the present annual migrations of birds is without much question certain physiological changes in the bird itself that have synchronized the season of the year and the life cycle of the individual until they serve as signals to start the bird on its northward or southward movements according to the season concerned. These physiological changes must be interpreted merely as seasonal alarm clocks that awaken the birds to movement. The original basic reasons for migration have unquestionably been different in the many different species concerned so that the underlying cause for migration as we see it to-day must be considered as due to a number of factors, some affecting one group of species and some another.

Man, through his physical clumsiness when compared to active birds, and through his dependence upon organization of his environment to enable him to live, moves from place to place only along certain established lines and through means of transportation that are limited in their scope and have many difficulties in their accomplishment. In contrast, the migrant bird, unencumbered by baggage, flies with few restrictions, finding food and shelter with comparative ease, traveling long distances with little apparent difficulty. The arrival and departure of our robins, swallows and wrens, particularly their return to our yards and grounds in spring, is a matter of pleasant observation on the part of many persons who are pleased to greet these friendly birds on their return from their winter's journeys. True appreciation of the great distances covered by these comparatively weak creatures comes however only to the naturalist traveling in distant coun-

tries who, seeing barn swallows crossing the seas that separate the main land masses of North and South America, or finding these birds circling over the open grass lands of the distant Argentine, or coursing over lonely lagoons in little known islands in the West Indies, marvels at the physical capability that carries these and others over such long distances, and puzzles over the necessity and the urge for such prolonged journeys.

Migratory movements among birds are easily understood in their performance in the case of large species as ducks and geese that fly by day and are seen crossing the sky in their northern and southern flights. Weaker forms as warblers, sparrows, flycatchers and other small species perform these travels by night so that they appear and disappear silently between suns, their coming and going frequently seeming a matter of deep mystery.

A few kinds of birds, when once established in an environment suited to their needs, have become wholly sedentary and move about within the narrow limits that suffice to provide them with food. As examples of such there may be mentioned the Carolina wren and our familiar bob-white or quail. There are others, as the blue jay, that are present throughout the year but may be represented in summer and winter by different groups of individuals as the species as a whole is distinctly migratory. Many tropical species living in areas where the temperature is equable throughout the year have regular times for appearance and disappearance, and seem to have regular migrations in that they shift from one haunt to another with seasonal shifts in food supply and other conditions in their chosen environment.

With most birds migratory movements are well known and their summer and winter homes have been recorded and outlined in detail with the dates of arrival and departure.

Some birds that nest in the Arctic tundras, as the golden plover, pectoral sandpiper, and the hunting gulls, or jaegers, in the winter season travel south to South America, or in the case of the jaegers even into Antarctic seas. Many of our warblers, flycatchers and vireos seek winter homes in Central America, northern South America and the West Indies, while bluebirds, robins and blackbirds travel only into the southern regions of the United States. Many of these latter pass only far enough south to escape the most severe weather so that they begin to return with the first warm days that come in February. The length of individual journeys in migration thus varies within wide limits, and in some species of birds that nest at high altitudes in lofty mountain ranges may be confined to a movement of a few miles to the plains about the mountain bases.

A few species of birds travel north after the nesting season to remain in areas north of where they nested and then pass south as cold weather comes. This is true of the snowy heron, the egret, and the little blue heron that nest in the southeastern United States and in late summer move north to the Ohio Valley, southern New England or even into southern Canada. The white forms of these herons appear along the Potomac River near Washington in July, and the birds remain until September in considerable numbers. The Wilson's petrel, or Mother Carey's chicken, rears its young on distant islands in the Antarctic area and in May travels far north beyond the equator through the seas of the world.

One of the most interesting facts in connection with migration is the almost clocklike precision with which it progresses in the temperate zones. Through records kept for long periods of years the average dates of arrival in spring and departure in fall for our common birds are well known, and it is found that they move with astonishing regularity varying only a few days from an

average date. More recently through the banding of birds, studies in which living birds are captured alive in ingeniously devised traps, marked with numbered aluminum bands so that they may be subsequently identified, and then released, it has been learned that the individuals of many wide ranging species have definite circumscribed areas for summer and winter homes and that apparently they move between these two along definite routes.

The chimney swift, called by many the chimney swallow, though it is not of the swallow family, resembling that group only superficially in form, in its southward migration gathers in great flocks in fall and is recorded abundantly in its movements south as far as the Gulf Coast of the United States. It then disappears to return the following spring. Years ago this swift was one of the species that, with swallows, the sora rail and some others, was supposed by the credulous to hibernate in a state of suspended animation beneath the water and mud of streams and swamps, and to revive and come to the surface again in spring. This superstition, entirely without basis, has been believed for centuries, and still persists in some localities. As regards the chimney swift, however, in recent years it has been observed in passage in eastern Mexico and in the Republic of Haiti, and it is believed that it spends the winter somewhere in the northern part of South America.

The bristle-thighed curlew, a large shore bird, is found in winter along the sandy beaches of Pacific islands from the Hawaiian group south to New Caledonia. In spring it comes north to Alaska and there disappears to seek some unknown breeding ground in the interior mountains of that territory. One by one however the mystery that has surrounded the homes of such birds is solved, and it will not be many years until the movements and ranges of all temperate zone species will be known.

The subject of avian migration is one of continual interest that will have its major contribution in coming years through studies aided by the bird banding just mentioned, which will finally explain many existing problems. The manner in which birds find their way in their long journeys is still one of the major points regarding migration that may or may not be solved, while the actual origins of our present day migrations, dating back as they must to beginnings in remote geological ages, since even our modern types of birds have been known for millions of years, will remain more or less mysterious. The subject is one that will repay study and consideration.

MARRIAGE AND FAMILY LIFE AMONG THE PLAINS INDIANS

By Dr. ROBERT H. LOWIE
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THE so-called savage tribes of the world are not like undisciplined hordes of apes. They do not live only to gratify their animal instincts. On the contrary, all their behavior is regulated by strict standards. The aborigines of Australia are among the simplest peoples on record, but neither in hunger nor in love do they act like the wild beasts of the forest. They abstain from what they consider delicious food because their elders so command, and they seek mates only from a definite class of girls or boys, respectively. In the old days breaking either of these rules would have been considered most immoral, and any

one marrying the wrong person would almost inevitably suffer corporal and often capital punishment.

What is true of these crude Australians holds in a general way for the Plains Indians of North America—the Sioux, Cheyenne, Crow, Blackfoot, Omaha and other buffalo-hunters. They did not control individuals by the laws of native Australians or citizens of the United States. But they had quite as fixed ideals of behavior, and a man's standing in his community depended on how closely he realized these standards in his personal life.

In some respects the practice of the Plains Indians resembled that of England under Queen Victoria. There was a double standard: women were expected to be virtuous, men to be gay blades. On the other hand, women who failed to live up to the highest standards did not become outcasts; they merely lost prestige and were barred from certain sacred ceremonies. Society was more charitable. It was also less hypocritical: the men did not profess a lofty morality which they did not practice.

Whatever principles, however, *were* accepted commanded unquestioning obedience. Blood-relatives did not marry one another, and where there was a clan organization even a tenth cousin or no kinswoman at all would be barred from marriage with a man who bore the same clan name. If a Crow, *e.g.*, broke this rule—a thing that hardly ever happened—he would not indeed be jailed or flogged or killed as in Australia, yet his punishment was severe from the native point of view. He lost standing in the tribe and was publicly exposed in songs poking fun at him at the very occasions when he wanted to make a grand showing. Even where clans were lacking, the norms were equally strict. If a young Cheyenne girl found that one of her suitors was related to her, no matter how remotely, she at once discouraged further attentions, and the young man would be greatly mortified to learn of his mistake.

How did a Plains Indian get a wife? One way was to elope with the girl of his choice, who was most frequently wooed while fetching water for her family. But such love matches, while not necessarily disgraceful, were rated as inferior. The proper procedure was for the suitor to offer property to the parents or brother of his choice, and these might accept or spurn the gift. At first blush this seems degrading to womanhood; the bride appears to have no say in the matter and to be treated as a chattel. But this is an unfair judgment. Indian girls were rarely forced into unions that were absolutely repugnant to them. They were guided by their parents for the same reason that white girls were guided by theirs a hundred years ago. Being married off at an early age—puberty—they were quite correctly believed not to know what was good for them. It is true that the parents in neither case know any better but they could not be expected to realize that. As for the idea of *buying* a wife, that was the greatest honor for the girl. If a young warrior risked his neck to capture ten head of horses he might offer as her price, it showed how much he valued her personality. He would not dream of making such an offer for a young woman who was not both chaste and competent. Then, again, there was generally rather an appearance of purchase than the reality. The girl's kin often gave back as much property as they received. The significant thing was an *exchange* of gifts between the two families. That exchange marked their sanctioning the new bond and cementing a relationship between two *groups* rather than two persons. The idea is still in vogue when a royal European prince marries in the interests of the State.

This notion that the *family* is mainly concerned in marriage—not the individual—explains two very wide-spread

customs. They held sway not only among our buffalo-hunting tribes but also in many other areas, in fact, they are recorded in the Bible. If a man died, his widow was often taken by a brother or cousin of the deceased. This evidently would maintain the old tie between the two families. Also, if a man took to wife the eldest daughter of a household, he often married her sisters also as they came to be of age. The Indians sometimes say that this is the way to enjoy polygamy without quarrels between the wives.

No woman felt disgraced by this or any other form of polygamy. However, polygamy could not be general for the simple reason that there were not enough women to go around. Hence, only the distinguished men could have two or more wives.

It might be supposed that where marriages were arranged by parents and where a wife could be inherited from an older relative, no such thing as true love could blossom. This is a grave popular error. Deep affection united many husbands and wives. There are even cases of "romantic love" among our Plains tribes. In the traditions that mirror their social life, we find cases of true devotion, a woman going alone through hostile territory to rescue a crippled lover; a man, who like Orpheus in Greek myth, follows his dead wife to the land of spirits.

There is another popular fallacy that an Indian wife was a much abused drudge. It is true that Plains women were more constantly employed than men, for they managed the daily routine. It was the wife's business to pitch and take down the tent, arrange the inside, cook food, fetch wood or water, dress the skins of animals and make clothing and containers out of them. The men did sometimes spend whole days in leisure. When they did work, however, their tasks were more arduous and either hunting buffalo or raiding the enemy was a dangerous job. As for maltreatment, an unfaithful wife might be severely mauled or even disfigured in some of the tribes, but generally a man was looked down upon if he abused his wife without strong provocation.

Children were dearly loved and coddled by their parents. The idea of beating them was very repulsive to the Indian mind. A white trader who lived with the Assiniboine for over twenty years never saw a man strike his child. Education proceeded along vocational lines, *i.e.*, parents gradually taught their offspring the pursuits of later life.

Some quaint ideas shaped Plains Indian family life. A Crow or Cheyenne was not permitted to talk to a grown-up sister; only as children were brother and sister allowed to play and chat together. Still stranger is the wide-spread rule—also found in Australia and Africa—that a woman must not speak to her son-in-law nor he to her, a rule which still survives. This has nothing to do with our mother-in-law jokes, but rests on a deep sense of mutual esteem. If a woman spoke to her daughter's husband, it meant that she had no respect for him.

These rules of etiquette surely make Indian family relations different from ours. But odd as they appear to us, they show refinement rather than brutality. They prove that social intercourse was not left to instinct but was strictly regulated by social norms. The Plains Indian was a stickler for the proprieties as he understood them. Neither as a lover nor as a spouse nor as a parent was he anything like the savage of popular fancy, but rather a human being like ourselves who happened to work out somewhat different standards of behavior while displaying much the same sort of human sentiments.

BEAUTY AND THE SCIENCE BEAST

By Dr. J. O. PERRINE

MONTCLAIR, N. J.

MANY clergymen, novelists, artists, poets, musicians and philosophers proclaim that the present era is lacking in appreciation of the beautiful and cultural. They bemoan the stifling and baneful influence of machines and science. They deprecate the last quarter century as debasingly materialistic, devoid of spiritual values. The pulpit and the writers of magazine articles would have us believe that only poems, songs, flowers, music, running brooks, waterfalls, mountains, the sea, ancient castles and ancient cathedrals are beautiful; that science is sinister, crushing and unlovely. Furthermore, the poet and his confrères boast of having a monopoly of beauty; they arrogate to themselves the possession of a patent on the spiritual and divine appointment as custodian of things cultural.

It seems to the scientist that beauty, after all, is of two kinds, macroscopic and microscopic. The poet's, the artist's, the clergyman's notion of beauty is macroscopic; to the scientist, beauty is microscopic. The poet enthuses over beauty as an overall attribute, as a composite of line, curve and color: a sunset, a forest primeval, a symphony, a Grand Canyon, a something of great proportions and awe-inspiring majesty; his is a general, a far-off, a long-distance view; he is a macroscopist. The scientist sees beauty in the individual item: in line, tone and color *per se;* an electron, the strength of a single wire among thousands which support a bridge across the Hudson River, the making of a glass stain to last for centuries in the windows of a Cologne cathedral; his is a particular, a close-up view; he is a microscopist. Those aggregates of line, curve, color and tone, which appeal to the eye and the ear, are the delight of the poet. His senses prompt him to write "A thing of beauty is a joy forever." That beauty of curve, color and tone exist is accepted by the scientist, but with his intellect he seeks to understand the "how" and the "why" and the "how much." Therein lies beauty for him. The poet sees a chapel at Trinity College, hears a Philharmonic; the scientist sees and hears, but facts and principles, inarticulately present in his mind, give him joy of a different kind.

The poet is prone to feel that detailed knowledge does not add to one's appreciation of the beautiful. He thinks this way because his sense of beauty is macroscopic. He says, "When Kreisler and Paderewski are playing, who wants to think of catgut and varnish and Stradivarius, and overtones, vibrations, steel wires and Helmholtz?" The scientist delights in thinking that from catgut and varnish and wood and steel, man evolved instruments on which man can play so charmingly. A subconscious recognition of the sheer mechanics of the artistry, an understanding of frequencies, vibrations and elasticities, afford a different sort of appreciation. Then, too, the scientist's mind tingles when he thinks of this same music's being carried to thousands who can not be in Carnegie Hall to hear with their own ears. Microphones, wires, coils, glass bulbs, all connected with leaden batteries, reproduce golden tones, because scientists like Helmholtz studied microscopically to bring about a result whereby poets in Indianapolis may listen macroscopically.

There need be no controversy as to

which type of beauty is more beautiful; each has its devotees; to each group its brand of beauty is very real and satisfying.

"When I consider the heavens, the work of Thy fingers, the moon and the stars which Thou hast ordained" was the poesy of a macroscopic psalmist. To gaze into the heavens and wonder and wonder and do nothing about it is one thing; merely to see Halley's comet streaming across the heavens is one thing; to concern oneself with the details of the heavens, with nebulae, with distances measured in light-years, is another thing. To understand the precision with which the planets speed along in their orbits; to think with Copernicus; to know that red stars are older than blue stars; to predict that Halley's comet will reappear at a specified hour, day and year; these facts, these achievements of man's intellect, these microscopic concepts are of themselves comparable in beauty to those of the psalmist; they add rather than detract.

A few years ago a poet, generically speaking, wrote as follows: "The best Christmas gift that has ever been given to North America is the establishment of friendly relations between the United States and Mexico by Lindbergh's flight. The reason for this is that Lindbergh has captured the imaginations of all people and has therefore gotten beyond cold scientific facts. The loveliest things in life are not scientific." What a foolish poet, what a narrow macroscopist, if such an antithetical phrase is tenable. Perhaps the imaginations of many people do not get beyond cold scientific facts; that is the way of most people. Fortunately, others have gotten deep into cold scientific facts—the Wright Brothers, Langley, Curtis, Lawrence the engine designer, oil chemists, geologists, experts in thermodynamics—all of them microscopists. Lindbergh's plane might have been made quite beautiful, in accordance with poets' standards, by having a modern artist paint an eagle on the fuselage, as Raphael painted a Madonna on the head of a wine barrel centuries ago. However, that kind of beauty would not have carried him to his goal. He sought every possible bit of fact, made most careful preparations, and his compass, altimeter, bank indicator, gauges, thermometer, tachometer and other microscopic beauties revealed to his intelligence, not his emotions, the required cold facts necessary to steer his way through darkness by night and above clouds by day. A flight to Mexico, a flight across the Atlantic; a telephone call from San Francisco to New York by wire, thence by radio to the Majestic in mid-ocean; a great locomotive speeding through a Moffat Tunnel; these are not accomplished because winds are favorable, electricity is docile, steam is tractable or rocks are soft. Accurate information as to whether the wind is propitious; how electricity and steam may be made to serve; sheer precision and analytical methods; these attributes of science are the factors responsible, and in them there is much beauty. The scientist agrees with the poet that the loveliest things in life are not scientific. However, they possess their loveliness in many, many instances because of science.

Flowers and children, all agree that they are beautiful. "Consider the lilies, how they grow. Solomon in all his glory was not arrayed like one of these."

> I met a little cottage girl:
> She was eight years old, she said;
> Her hair was thick with many a curl
> That clustered round her head.

The poet says: "Don't pick the lily to pieces; don't think of its stamens and pistils, its leaves; don't destroy it; just look at it and be charmed by its

beauty." If a lily is blighted, he says "How sad!" He may write a poem about it. The botanist analyzes. He considers how the lily grows and he studies plant diseases. If a field of lilies is blighted, he studies the blight. More lilies, and more beautiful, result from his understanding. The chemist, another microscopist, learns how to prepare the soil to better grow lilies. He and the botanist see beauties unknown to the poet. They have not impaired beauty by analysis. They have multiplied beauty.

Vitamins! What an achievement for the microscopist! Mother love is beautiful, but not sufficient in itself to restore health to a sick child. For generations and generations, children and mothers have been admired and written about and extolled by poets, novelists and parsons. Such macroscopic admiration never saved a child's life or restored one to health. Vitamins, literally a product of test-tube, analysis, microscope and cold scientific fact, help make sound bodies for sound minds.

A vial of insulin may not possess the color of one of Wordsworth's daffodils or the fragrance of one of MacDowell's wild roses, but it does give life, and, after all, to have life and have it more abundantly is most beautiful. To see the beauty in a vial of insulin, one must look deep and look imaginatively. Insulin is, because a scientist studied dumb animals microscopically and found another kind of beauty than is seen in pictures of them by Paul Potter.

Some years ago, France made an appraisal of its famous men and women. Among those in its Hall of Fame the greatest was to be chosen. Who was chosen? A novelist? A statesman? A soldier? A painter? No. A microscopist, Pasteur. Mindful of the good he had done and the happiness he had given to the world, the common folk acclaimed him the greatest. If one were to be added to the list, perhaps that of Madame Curie would be added. Madame Curie, microscopist of microscopists. Bacteria—tiny and invisible. Radium—tons and tons of ore to produce a tiny speck of radium, from which the tiniest of tiny rays radiate with healing in their wings.

It is a custom in the United States to use superlatives. Bobby Jones is the greatest golfer of all time. Other greatest of the greats have been named. What if we would do as France did and seek the greatest of our great men. Would he be a statesman, a poet, a surgeon, a painter, a preacher, a soldier, a novelist, a musician or a scientist? Would he be a macroscopist or a microscopist?

The smokestacks, coal piles, whirling electric generators, spray ponds and boilers, which go to make up a great electric light and power station, are not generally regarded as suitable subjects for polite and cultured conversation. However, to discuss the irregularities of French verbs, the use of the subjunctive by Shakespeare, the prevalence of red in Louis XIV furniture and similar recondite topics, is at once to identify oneself as a cultured guest. To know about or even be aware of the existence of a physical structure that sends out light to make homes cheery and attractive, that sends out heat to make homes comfortable, that sends out power to factories where men can work to make a living; that is a breach of good form, according to the poet's concept of culture. The scientist's, the microscopist's, sense of culture prompts him to say that to be ignorant of contemporaneous factors which help make life healthier and happier on the purely physical side is to lack culture of the finest and broadest type. To be well versed in Plato, the Renaissance, English lyric poetry, transcendentalism and modernistic art, and not have the

slightest idea of or concern in those things (the word "things" being used advisedly) which enable man to enlarge and enrich his life, is equally as uncultured as he who lives by bread alone.

In "Les Misérables," Victor Hugo relates an interesting conversation between the bishop and his gardener. The gardener has made plans for the garden and says to the bishop, "Here, I will plant cabbage; there, I will plant carrots; yonder, I will plant lettuce." After plans for the garden have been completely described, the bishop remarks, "But you have left no place for flowers." The gardener replies, "We must not waste any land for flowers; they are not useful." The bishop rejoins, "The beautiful is as useful as the useful; at times, more so."

The scientist subscribes to this epigram of the bishop, and is not one of those who believes money wasted when it is used for cathedrals, symphony halls, art galleries, libraries and parks. However, the scientist desires to point out to the poet, the artist and the bishop a corollary: "The useful is as beautiful as the beautiful; at times more so."

A poet has said, "When we see something beautiful, then we worship." The scientist says, "When one sees something beautifully useful, then it is also time for worship."

Much is said and written about world progress and development. There have been many contributory factors in the past and there will be many different factors in the future. Science and the microscopist will do much in the realm of detail and analysis in doing the work of the world and in making it a better place in which to live. The macroscopist will continue to write poetry, paint pictures, preach sermons and write music, but with all will not have a corner on beauty and culture, nor on things of the spirit.

THE STARS ON OUR FLAG

By AUSTIN H. CLARK

U. S. NATIONAL MUSEUM

FROM the bottom of the sea instead of from the sky came the stars that represent the states upon our flag, if it be true, as is commonly believed, though by no means proven, that the idea of the stars and stripes was suggested by the coat of arms of the Washingtons of Northamptonshire. Furthermore, these stars, originally formed at the bottom of the sea, for millions of years lay buried in the earth before they were first seen by the eye of man.

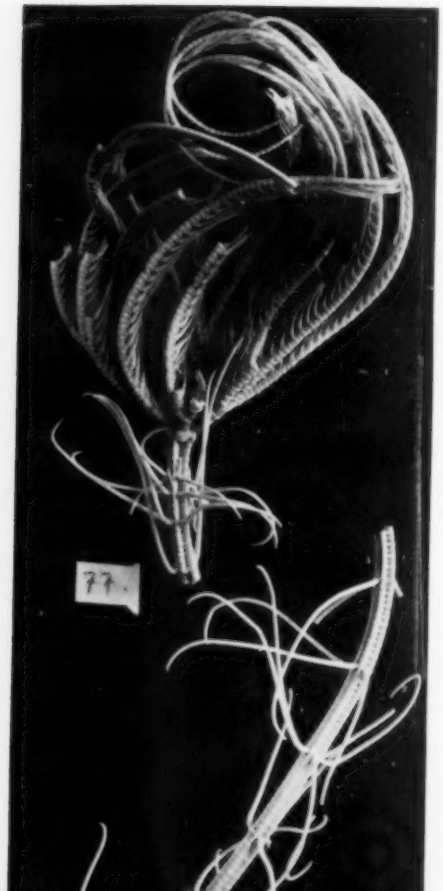
THE WASHINGTON COAT OF ARMS

On the surface of the soil in the Midland shires of England there are often found little five-sided or star-shaped stones which are beautifully and intricately sculptured on the two flat sides. Similar little stones are found in many other places. Sometimes these little stones are circular instead of star-shaped. The central hole permits them to be strung like beads, and in ancient times they were often used for rosaries. They are locally variously known as St. Cuthbert's beads, rosary beads, giants' tears, fairy stones, wheel-stones, star-stones, entrochites and asteriae.

Wherever they are found these little stones always attract attention. Their regular form and exquisite sculpture set them quite apart from all other kinds of stones. Usually they are regarded, or were regarded in the past, with superstitious awe as something supernatural, or at least uncanny. This was especially true of the star-shaped stones.

Because of the superstitious awe with which they were regarded by the common people the star-shaped stones quite naturally appeared upon the coats of arms of the important families in the regions where they are to be found.

In the language of heraldry the star-shaped figures representing the little star-shaped stones are known as *mullets*.



A LIVING CRINOID

WITH A STEM COMPOSED OF AN IMMENSE NUMBER OF PENTAGONAL AND STELLATE JOINTS; FROM THE CHINA SEA IN DEEP WATER.

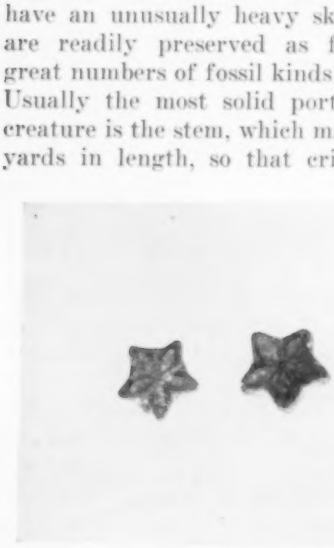
Just as the little star-stones may either have a central hole or be without one, so the heraldic *mullets* may be simple stars or stars with a central hole known as *pierced mullets*.

The word *mullet* is derived from the French *molette*, a diminutive of the Latin *mola*, a millstone, used for various wheel-like objects. In heraldry the *mullet* is one of the common marks of cadency, and is used to indicate the third son.

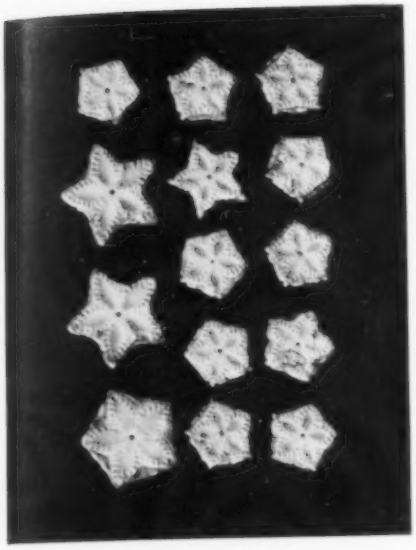
The coat of arms of the Washingtons is technically described as "argent, three bars gules, in chief three mullets of the second." In other words it bears three figures representing star-stones.

Now just what are star-stones? Star-stones are the detached and isolated joints of the long stems of peculiar creatures called crinoids or sea-lilies that live on the bottom of the sea. They are related to the common star-fishes found along the shores. The star-stones in the soil have come from sea-lilies that in the far distant past lived upon a sea bottom which has been turned to stone and is now lifted above the surface of the sea. They have been freed from the rock and separated from each other by the disintegration of the rock through the action of rain and heat and cold.

There are very many different kinds of sea-lilies, and some of them in certain places are enormously abundant. Because of the fact that these creatures have an unusually heavy skeleton they are readily preserved as fossils, and great numbers of fossil kinds are known. Usually the most solid portion of the creature is the stem, which may be many yards in length, so that crinoid stems



STAR-STONES FROM FRANCE



STAR-STONES

FROM A LIVING CRINOID DREDGED IN DEEP WATER IN THE PHILIPPINES.

and stem-joints are among the commonest of fossils.

From the present seas we know between six and seven hundred different kinds of crinoids, and of course many more remain to be discovered. They live in all seas everywhere, and are found from the tide pools down to 17,400 feet, or about three and one-third miles, beneath the surface. Most of the still living kinds have stems only when young, at a certain age discarding them. But about one hundred kinds keep their stems throughout their lives.

Those that keep their stems have stems of various kinds, with the joints long or short, circular, pentagonal, or sharply star-shaped. At the present time those with star-shaped stem-joints are to be found only in the Caribbean Sea and Gulf of Mexico, among the Malayan Islands, and along the coasts of eastern Asia.

In North America star-stones are known only from a few localities in the west and extreme northwest, and are quite uncommon.

The history of the stars upon our flag, which have risen slowly upward from complete obscurity in the ocean's depths to the surface of the land and have then been raised aloft, is curiously parallel to the history of the great idea—representative democracy — completely suppressed in early times, then dormant in men's imaginations, now raised aloft and in full flower, upon which our government is based.

This history, however, assumes a definite connection between the stars and stripes upon our flag and the mullets and bars upon the coat of arms of the Washingtons, for which there is no real evidence. In the history of the flag the stars are referred to always as stars, never as mullets, though their form is exactly that of the heraldic mullets.

THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

THE PROGRESS OF SCIENCE

THE NEW RESEARCH LABORATORIES AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

REALIZING that the law of physics and chemistry are the basis of engineering, and believing that the future will see great advances in the art of applying science to human welfare, the Massachusetts Institute of Technology has taken a significant step in preparing for these developments by the building of new research laboratories for physics and chemistry. Being built from a fund provided by Mr. George Eastman, these laboratories are named "The George Eastman Research Laboratories."

The main laboratory is a building 300 feet long, varying from 60 to 75 feet in width, and five stories high, including basement. One half of this building is assigned to advanced work in physics and the other to advanced work in chemistry. Adjoining and connected to the physics side of the main building is a specially designed spectroscopy laboratory about 100 by 60 feet and two stories high. The buildings are faced with limestone and are of reenforced concrete construction.

These laboratories have been designed with great care to minimize disturbance

THE GEORGE EASTMAN RESEARCH LABORATORIES
NEARING COMPLETION WHEN THIS PHOTOGRAPH WAS TAKEN IN MARCH.

GEORGE EASTMAN

WHO PROVIDED THE FUNDS FOR THE LABORATORIES, BUT DIED BEFORE THE BUILDINGS WERE COMPLETED.

from vibrations arising either within the buildings or transmitted from the outside. The entire spectroscopy laboratory and also several research rooms for physical chemistry are enclosed by thick layers of cork for heat insulation. This is so effective that the most violent changes of temperature would not affect the inside of the rooms by a rate of more than 1 degree in about three months. Special temperature controls and air-conditioning arrangements are provided.

Two features of the new laboratories are deserving of special mention. One is an unusually adequate and convenient arrangement of a departmental library for physics, chemistry and mathematics, with a reading room, stack rooms and a large number of small cubicles with desks for study in the stack rooms. Adjoining this library are a series of small offices for men engaged in research work of a theoretical nature.

The second special feature is a very attractively furnished social room with an adjoining kitchenette, which has been made possible through the generosity of Mrs. Forris Jewett Moore, and is designed to make the study of chemistry and the allied sciences more attractive. This room will serve as a social headquarters for the advanced students and faculty of the chemistry and physics departments, and will thus provide a type of educational contact which is coming to be recognized as very advantageous.

With the aid of these splendidly designed and equipped laboratories, the Massachusetts Institute of Technology hopes to make a significant contribution to the future development of pure and applied science.

KARL T. COMPTON

THE BATTELLE MEMORIAL INSTITUTE

A MEMORIAL laboratory endowed with sufficient funds to carry on an extensive program of research in the fields of metallurgy and fuels and yet offering facilities for research sponsored by industry or individuals is indeed a unique institution.

The possibilities of such an institution were conceived by Gordon Battelle, a young industrialist who died in his early forties a number of years ago. By his will he established Battelle Memorial Institute as a research institute directing that it specialize in metallurgy, fuels and allied fields. He also directed that it be staffed with experts in the chosen fields and equipped to conduct a program of research on its endowment funds, and have facilities for research by others available at nominal cost well within the reach of all.

His idea of such a dual functioning was well founded. It enables the staff to have the direct broadening contact with industry that is essential to good work in these fields. Without such contacts any group is apt to become ultra-scientific to the point of overlooking the more practical aspects of research. It is because of these features that Battelle Memorial Institute has been said to be among the ideal institutions for research in this country.

The memorial research laboratory, completed about a year ago, is modern in every respect and provides some 90,000 square feet of floor space especially designed for the purpose intended. The architecture is of the Roman Ionic type effected by a pleasing combination of brick and limestone. The building is located on King Avenue adjacent to Ohio State University where contact with the scientific atmosphere of this institution is helpful.

The first floor of the front section of

the building is memorial in character with spacious entrance and lobbies done in beautiful architectural treatment with polished marble walls and bronze fixtures. The balance of the dignified structure is devoted to the practical operation of research work. The equipment, which is constantly being added to, is of the most up-to-date type. The equipment purchases are oriented in relation to the projects carried on by the institute as well as to those sponsored by industry.

The institute's staff has rapidly grown in the past year to a total at the present time of sixty, the technical staff being

THE BATTELLE MEMORIAL INSTITUTE IN OHIO

specialists in various phases of metallurgy, fuels or allied fields.

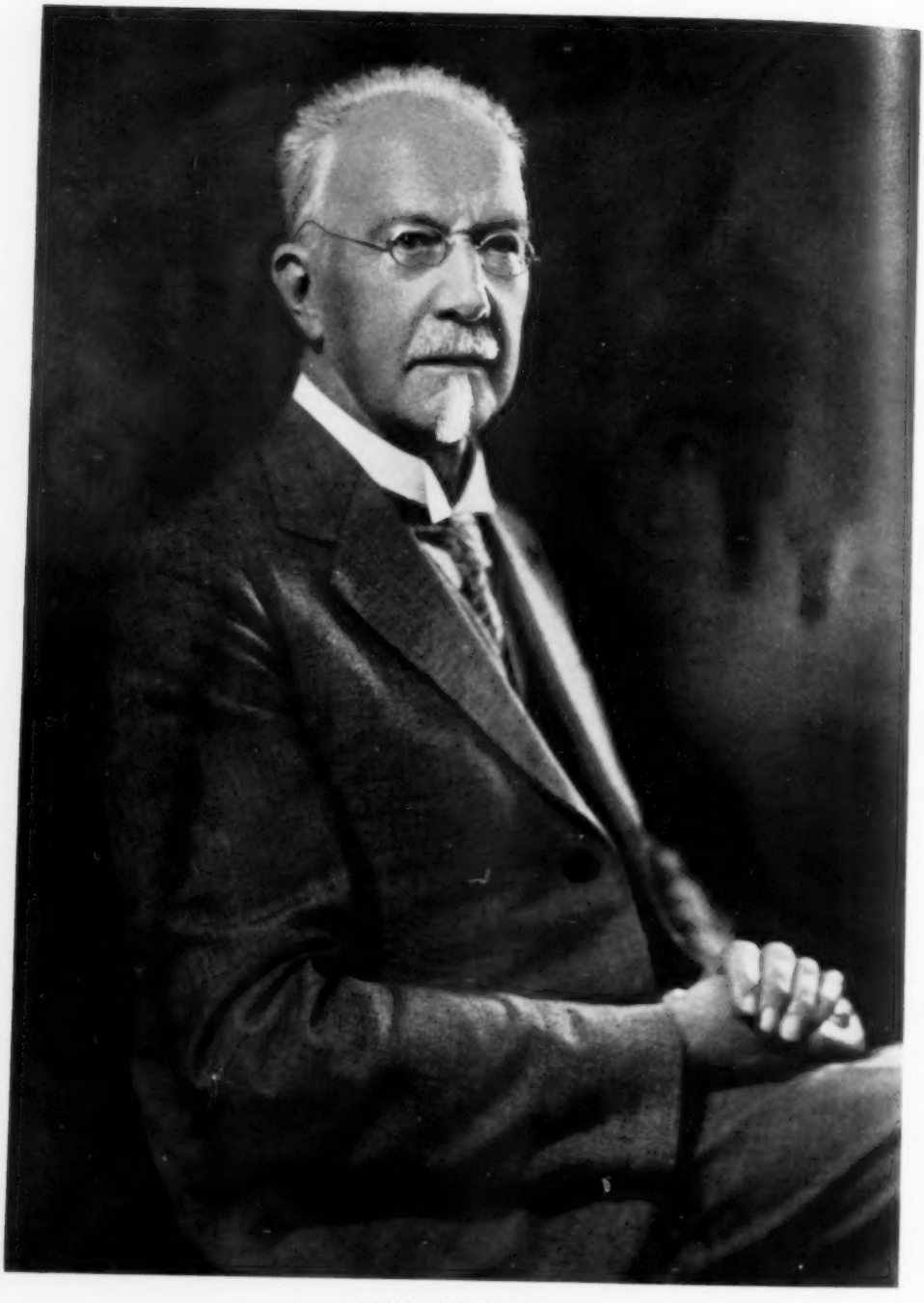
The response from industry and individuals during the first year of actual operation has well demonstrated the fact that the facilities at Battelle Memorial Institute are in active demand and its capacity for industrial work will soon be taxed if the present rate of growth continues.

The following projects, sponsored by industry, are in progress: Endurance properties of special steels; research on the beneficiation of low-grade iron ores; research on cast iron; special developments for the use of metal foils; research on bearing metals; research on alloys for the production of thin metal sheets; research on thermal properties of metal; editorial work on "Metals and Alloys"; research on metal roofing materials; study of embrittlement of structural steel in galvanizing.

Eight of the major projects supported in whole or in part by the institute funds are indicated below: Centrifugal methods of metallurgical analysis; combustion of pulverized fuel; low-temperature properties of aircraft material; low-temperature carbonization of coal (cooperative with Engineering Experiment Station, Ohio State University); alloys of iron research (codification of known information and preparation of monographs, cooperative with Engineering Foundation and ten technical societies); miscellaneous nitriding problems; constitution of hydrocarbons; effect of coal ash on refractory brick (cooperative with the American Society of Mechanical Engineers and the U. S. Bureau of Standards).

THE ENTRANCE HALL

THE LIBRARY



MAX VON FREY

MAX VON FREY

Max von Frey, professor of physiology and director of the Physiological Institute of the University of Würzburg, died at Würzburg on January 25, 1932. Born at Salzburg on November 16, 1852, he was thus in his eightieth year; and although in poor health he continued his researches and lectures until a few weeks before his death.

After completing his medical studies von Frey was associated with Karl Ludwig at Leipzig and E. Hering at Zürich. In 1899 he was called to Würzburg to be the successor of Adolf Fick.

The investigations of von Frey fall into three periods. An early period of study of the mechanics and metabolism of muscle, which, begun as a student, was continued until the results justified inclusion in Nagel's Handbuch der Physiologie, under the title "Allgemeine Physiologie der quergestreifen Muskeln." One important observation of this early research was that lactic acid could be oxidized in the liver. The use of physical methods in physiological investigation was keenly appreciated by him. Thus, in the second period, he employed such methods in prosecuting studies of the circulation, the results of which are contained in a monograph "Ueber den Puls," which he dedicated to his teacher, Ludwig.

From 1895 to his death he was engrossed in the problems of cutaneous sensations, his life work, and a phase of physiology with which his name will always be associated. He was intensely interested in the skin as the boundary between the organism and its environment, and in the manner in which the environment impresses itself upon the organism through the nervous pathway. Employing stimuli of just threshold value, he demonstrated that the various sense spots were discrete elements. As early as 1895, on purely logical grounds, he indicated that the end-organs of Krause were associated with sensations of cold, those of Ruffini with warmth. He demonstrated the existence of a deep pressure sense and showed its significance in an appreciation of posture and movement. The results of these investigations upon cutaneous sensations of von Frey and his students are contained in thirty volumes of "Arbeiten aus dem Physiologischem Laboratorium der Würzburger Hochschule."

In the "Vorlesungen über Physiologie" the materials and methods of many years of instruction of medical students are woven into a volume which has the merit of clarity and brevity without the sacrifice of essential material.

In the conduct of his own researches he imposed upon himself the necessity of a high degree of accuracy and exactness, and the conclusions he drew were only those that were warranted by the results of the experiments. He demanded of his coworkers the same carefulness and accuracy. As a result of his training many of his former students fill responsible positions in the German universities.

In both appearance and bearing, Professor von Frey was very dignified. Although somewhat reserved, his students knew that his was a kindly and warm nature. He was interested in the welfare of his assistants as well as in their research problems, and it was always possible for them to seek advice of him. Those American students who sought permission to study at his laboratory were certain of a warm and generous welcome.

A master of his subject, a seeker for the truth, a great teacher has passed.

Bartgis McGlone
Würzburg, Germany

GUESTS AT A DINNER IN BERLIN GIVEN BY PROFESSOR VON LAUE TO DR. MILLIKAN

PROFESSOR WALTER NERNST, PROFESSOR ALBERT EINSTEIN, PROFESSOR MAX PLANCK, DR. ROBERT A. MILLIKAN AND PROFESSOR MAX VON LAUE

WEIGHING THE ATOM

A NEW precision mass-spectrograph, the instrument by which physicists actually weigh individual atoms, was described to the members of the American Physical Society at their meeting on February 25 in Cambridge, Massachusetts. The instrument had just been completed by Dr. Kenneth T. Bainbridge, of the Bartol Research Foundation. Until now there has been only one such instrument of high precision in the world, that of Dr. F. W. Aston in England. He was recently knighted for the important discoveries he made with it. The new instrument brings to the American world of physics a research tool of immense value.

In spite of the smallness of the atomic particles, it observes the new mass-spectrograph is a large and complicated construction occupying with its auxiliary apparatus a large portion of an ordinary room. The atoms it weighs are only about 10^{-25} ounces in weight, but the machine itself weighs over two tons. It required nearly a year to build it, and, in spite of every economy in the use of home-made parts, represents an outlay of over $2,000, exclusive of Dr. Bainbridge's time.

The importance of a precision mass-spectrograph lies in its ability to furnish information concerning atomic nuclei—now in the center of the research limelight. It is thus complementary to the researches with high voltages and fast particles now in progress under such experimenters as Professor E. O. Lawrence, in California, Dr. M. A. Tuve and his associates in Washington, D. C., and Dr. Robert J. Van de Graaff at the Massachusetts Institute of Technology. While these observers seek to break down the atomic nucleus to learn its secrets, the mass-spectrograph studies the nuclei as they are.

Many problems await Dr. Bainbridge's attention, but perhaps the most important of these is the determination of the exact mass of the newly discovered isotope of hydrogen. This new kind of hydrogen atom is known to weigh approximately twice as much as the ordinary hydrogen atom. Beyond that fact nothing is known about it, and indeed its very existence was only proved within the last three months by the recently reported experiments of Professor Urey and Dr. Murphy at Columbia University and Dr. Brickwedde at the Bureau of Standards.

When the new hydrogen is available in concentrated form, Dr. Bainbridge will determine its so-called "packing fraction." The atomic nucleus in this case is supposed to be made out of three elementary particles, one electron and two protons. These are expected to weigh a little less in combination than they do when they are separate. The difference is called the packing fraction and it represents the amount of mass which would be lost in making a new type hydrogen nucleus out of its elements.

Out of the Einstein theory of general relativity comes a strong suggestion that mass lost in such atom construction can appear in the form of radiant energy. It would produce rays like the cosmic rays studied by Millikan and others which are now being investigated widely and systematically by Dr. A. H. Compton. Millikan is convinced that this is the true explanation for the existence of cosmic rays; only he has had to refer to the building of rather complicated nuclei partly because he was unaware of the existence of the simple new hydrogen type. The nature and penetrability of the cosmic rays emitted would depend upon the amount of the packing fraction

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which, in turn, would determine their wave-length. Hence it will not be possible to relate the new hydrogen nucleus to the observed cosmic rays until its packing fraction is determined.

This will be a simple matter for Dr. Bainbridge as soon as he receives a suitable sample of the hydrogen. In fact, his apparatus works best for the lighter elements like hydrogen, and the paradoxical situation arises that single atoms of elements like gold and mercury are actually almost—but not quite—too heavy to be separated and weighed conveniently!

Other chemical elements than hydrogen will come under Dr. Bainbridge's observation, for Aston has shown that most elements consist of a mixture of isotopes, or atoms of different weight, and many elements have not yet been analyzed from this point of view. In the case of these elements we actually have no idea of the weight of their atoms, even though their "atomic weights" have been accurately determined by the chemists. The situation is like knowing the average weight per orange of a bag of a dozen oranges, easily found by weighing the bagful, but not knowing the weight of any one of the individual oranges in it. Only when the individual weights of all the different kinds of atoms of all the various chemical elements are known, will physicists be satisfied. In the 92 elements of a chemists' periodic table, well over 150 different isotopic species have already been found and the search is far from finished. Thus the physicist with his more particular eye discerns a greater variety of atomic bricks of matter than the chemist has needed to take into account.

To explain the operation of the mass-spectrograph in discovering the isotopic constitution of an element or determining the packing fraction of a nucleus, Dr. Bainbridge made the following statement:

"Ions of the elements to be investigated are produced by an electrical discharge through the vapor of the element or a volatile compound of the element or by other means by which one or two electrons can be removed from the outside of the atoms in the discharge tube. The resulting ions are of all different velocities and energies and a wide range of masses, depending on what elements were present. The beam of ions produced in the discharge tube is introduced into a chamber which may be called a velocity filter. The property of this section of the apparatus is to allow through into the final part of the apparatus ions of a definite velocity of all the various masses. The beam of ions leaving this part of the apparatus is introduced into a magnetic field and bent around 180° and brought to a focus on the surface of a photographic plate. The ions describe circular paths with radii proportional to the mass of the ions, so that the heavy ions are focussed on one end of the plate and the lighter ions are focussed on the other end. The photographic plate is darkened at the points of impact of the ions. By careful measurement of the plate it is possible to determine the masses of the ions and hence the masses of the nuclei of the elements with an accuracy of one part in 10,000. By measuring the blackening produced by the ions the relative abundance of the isotopic constituents of an element can be determined. "The actual mass of the material incident on the photographic plate can not be greater than 10^{-12} gram for the usual exposure of a mass spectrum of two or three elements consisting of 5 or 6 atomic species. Exposure times range from 30 seconds to 15 minutes. The energy of the ions ranges from 5,000 to 20,000 electron-volts."